

How Not to Operate a Telephone System

AFTER two years of negotiation in Washington, the Intelsat organization has this week finally reached an agreement on a form of organization for the continuing future or, more specifically, for the next six years. The difficulty in reaching the new agreement is a reflexion of the genuine conflict of interest between most of the member nations of Intelsat and the United States and its associates. The agreement which has now emerged will do something to satisfy the requests of the majority that Intelsat should not be permanently dominated by the American corporation called Comsat, which is at present not merely the United States delegate to Intelsat but the technical organization formally contracted as the manager of the developing international system of communications satellites. Comsat will remain the manager for the next six years, but there is to be a secretary-general to look over the manager's shoulder and also a long-term study of the problem of managing Comsat so that, six years from now, it will be possible to invite tenders from all kinds of interested parties to become the manager or even to think of some entirely preferable system of operating the complicated and crucial part of the international telecommunications network which Intelsat will by then have become. The measure of the dissidents' victory in the protracted negotiations in Washington is that it has not been necessary to agree that Comsat should be permanently the technical manager. The common failure in the new agreement is best represented by the simple truth that six years is a long time by anybody's reckoning, but especially in telecommunications.

The case against Comsat is worth recounting if only because the principles involved are important for all kinds of international organizations. Intelsat itself was originally conceived of as an international and permanent conference of governments that would seek to make the fullest use of communications satellites. The need for such an organization follows directly from the way in which communications satellites are plainly incapable of regulation by means of the bilateral agreements which are traditional in international telecommunications. For the first few years of its existence, however, Intelsat seems to have been an entirely pragmatic association of the familiar international carriers—organizations such as the post offices of Western Europe but with Comsat replacing private organizations such as AT&T. In circumstances like these, it may have seemed entirely sensible that Comsat, with its access to the expertise of the National Aeronautics and Space Administration, should have emerged as the technical manager. Most of the participants in Intelsat seem to have been exceedingly slow to wake up to the snags in the arrangement, not the least of which is that Comsat is not merely a servant of Intelsat, but a commercial organization with a responsibility to its shareholders to make a profit. It should also, however, have been clear that there must be serious difficulties in any arrangement in which an American organization, acting as manager, would be charged with responsibility for letting commercial contracts for the development and

construction of satellites which must, at least at the beginning, be awarded largely to companies in the United States. To make things worse, the founders of Intelsat appear to have been so anxious to delegate to the manager responsibility not merely for day-to-day management but for long-term planning of all technical aspects of the international satellite system, that they found themselves not merely relieved of humdrum chores but robbed of responsibility as well. The result, painfully apparent at the start of the negotiations in Washington two years ago, was an arrangement that promised to become increasingly inequitable as the years went by.

It is only fair to acknowledge that Comsat has been, if anything, more consistent than the members of Intelsat which have now become its critics. To expect that Comsat would have bowed gracefully to the wishes, frequently unspoken, of the members of Intelsat that some other method of managing the enterprise would be found has always been an assault on reality. Moreover, it has always been clear that the special virtue of having Comsat as a manager was that only an organization with the full backing of the NASA, American corporations such as Hughes Aircraft and even the Department of Defense would have been able to make Intelsat a reality. However unpalatable this knowledge may be, the members of Intelsat owe a considerable debt to the taxpayers of the United States who have paid for the research and development from which the Intelsat satellites have sprung. Hindsight may suggest that it would have been better to have paid in cash and not in kind for the privilege. Certainly the members of Intelsat should now regret the haste with which they signed an agreement that had all the makings of trouble. But six years is a much longer period of security than Comsat would have won if Intelsat were the thoroughly commercial enterprise it sometimes considers itself to be. One of the practical difficulties in the new arrangements is that the six years ahead will see designed and built the generation of communications satellites likely to determine the pattern of the international satellite communications network for the eighties and beyond. A more subtle problem is that the years immediately ahead are likely to provide all kinds of temptations for regional communications systems to be developed alongside Intelsat, and these tendencies are likely to be powerfully reinforced if there is continuing trouble over the management of Intelsat. It follows that much will depend, in the years ahead, on the success with which the new secretary-general can establish a friendly but firm relationship with Comsat.

But what is to happen when Comsat's six-year lease is up? One of the most depressing features of the agreement now negotiated is that the members of Intelsat appear to be reconciled to yet another wrangle over the leasing of this franchise in 1977. By then, of course, the problems will be even greater than they have been in the past two years. First of all, there will be several other contenders, including private companies in the United States but even possibly government agencies. The field will



by then be confused by foreign entries, however, and it will then be hard for Intelsat to resist the notion that it too should practise Buggin's term and award the contract for management to some American organization. In all these circumstances, the task now facing the working party and the management of Intelsat seems plain enough. Whatever the new agreement may say about the desirability of putting as much as possible of the work of the

organization out to contract, there is a great deal to be said for some device that will allow Intelsat the freedom to build its own infrastructure, to employ its own staff, to buy and install its own equipment and even to carry out its own research and development. Given the pace at which negotiations have crept along in the past two years, it is by no means too soon to start working towards that end.

Small Success at SALT

THE announcement last Friday by the American and Russian governments that there has been some measure of agreement in the latest round of negotiations on strategic weapons is a sign of the willingness of both sides to reach some kind of an agreement. The question is whether it is more than that. The two governments have apparently decided to spend their time at the SALT talks negotiating an agreement on the limitation of anti-ballistic missiles coupled, if necessary, with an agreement on the employment of offensive weapons. To the extent that the two governments have found something in which to sink their teeth, this is of course a welcome step. For the Americans, the decision to look first for an agreement on ABMs is a particular success, for the Soviet government has for the best part of a decade professed to be puzzled to know how a weapons system designed to protect a

population from attack can be held to be more inflammatory than an offensive weapons system pure and simple. The truth is, as has been clear since 1966, that anti-ballistic missiles are dangerous weapons not because of their potentially protective power in saving lives but because of the way in which any substantial immunity from attack which they might confer would undermine the reality of strategic deterrence. Because this is such an obvious point, and because it has been so well publicized in the past few years, it is of course a disappointment that the two super-powers have only now arrived at this discovery. But that, fair play, is the stuff of which diplomacy is made. The first question to be asked now is whether a decision to negotiate on the limitations of anti-ballistic missiles can be expected to lead to better things.

Cooperative Research in Trouble

THE industrial research associations are a particularly British invention (dating back to the twenties) which seems now to be near its end. In the House of Commons last week, Mr David Price told the Select Committee on Science and Technology (see page 208) that he would be scrutinizing carefully the terms of government grants to the research associations whenever they came up for renewal. He said that his department was attempting to work out new policies for the research associations "with the object of establishing priorities" (whatever that may mean) and of finding out which associations "justify continued public support". To the extent that Mr Price implied that new policies mean less generous policies, his view is at least more open than that of the last government, which tried to forget about the research associations by laying down the principle that the total cost to the Exchequer should not exceed £4,000,000 in any year. For all its enthusiasm for industrial research, the Labour Government found itself quite out of sympathy with the research associations but yet unable to put them out of business. For quite different reasons, Mr Price and his colleagues seem also to be disaffected from the associations but nevertheless show much more concern with a level of public expenditure. The question is whether meanness towards the research associations, already reduced to 80 per cent of their allocation from Mr Wedgwood Benn, will be a gain or a loss.

Half a century ago, research associations seemed to be a sensible device for helping industries to acquire a modest amount of scientific skill and advice at moderate cost and without much risk. Over the years, the device of

setting up a research association turned out to be an especial convenience in industries which had fallen on hard times. This was the spirit, in the years after the Second World War, in which the government tried to stiffen the commercial competitiveness of industries such as cutlery and shipbuilding by offering grants going a long way towards the cost of cooperative research establishments. Part of the hope behind these adventures was that the laboratories thus created would carry out work directly useful to the research association's members, but it was also hoped that these establishments would become ways of interesting the scientific and technical community as a whole in novel problems, ways of training people in new techniques and, with luck, demonstrations that research is neither an extravagance nor a waste of time but the source of all kinds of valuable innovations. In most circumstances, the government's expressed intention in setting up a new research association has been to devise a formula by means of which its own contribution to the cost would diminish steadily over the years. In this event, it has usually been necessary for the government to keep on paying money each year long after its original enthusiasm has evaporated. This is one sign of how frustrating the experience of successive British governments has been. This is also one ready explanation of how the tide has been running against the associations for the past five years.

As well as being a drain on public funds, however modest, the research associations have also been a thoroughly unpredictable collection of public institutions. Some of them, such as the British Iron and Steel Research

Association (now under the wing of the British Steel Corporation), have always had splendid reputations as scientific establishments. Others have been equally distinguished by their incompetence. Some years ago, for example, the British Food Manufacturers' Research Association turned out to be making inadequate use even of the inadequate grant which its members and the government were between them assembling. Still other research associations have quite properly said that neither research nor development was as necessary to their members as the provision of a decent information service, with the result that they have set out to provide technical advice and information of the kind which can be provided by a team of skilled technical librarians backed up by only the most rudimentary research programme. While some of the variations of kind between one research association and another may be explained by the circumstances of different industries, the quality of the research establishments created under the programme must also have been intimately dependent on the quality of the people available and on the willingness of member companies to take a long view about the return to be expected from investment in research. It is also with hindsight clear that some of the research associations were regarded by member companies not as potential sources of innovation but as stalking horses by means of which the pace of change throughout an industry could be observed and understood. In short, the research associations have understandably been all things to all men, and there is no easy generalization to be made about them.

In circumstances like these, it is easily understood that the government should take a mean view of the future of the research associations. As they have turned out, the research associations are an untidy and even an unruly set of institutions. No government can be expected to be fond of them. A passion for tidiness is not, however, a sufficient reason for deciding without public argument to run down the government grant to the associations. It is a misfortune that a select committee this week seems not to have been in the mood to ask Mr Price for chapter and verse on the decision at which he and his colleagues have apparently arrived. To be sure, everybody now knows that the Department of Trade and Industry is dedicated to the view that industry must stand on its own feet, and in this spirit the removal of the government grant from the research associations is presumably intended as an indication to the member companies to increase their share of the cost—or to decide that the benefit of membership is not worthwhile. In reality, however, the £3.25 million which the government is this year spending on the research associations is a small part of this expenditure on research at the public laboratories which it controls—institutions such as the National Physical Laboratory, the Warren Spring Laboratory and the several laboratories of the Atomic Energy Authority. It is important that somebody should ask whether the industrial benefit, pound for pound, of what the government spends on the research associations is more or less than what it gets from the continued support of its own laboratories in the condition to which they have, over the years, become accustomed. Without further evidence in public, Mr Price and his colleagues are the only people who can make this study and they must therefore shoulder the responsibility for seeing that it is properly carried out.

The new policy on research associations also provokes

the question whether the new government has yet made clear to itself, let alone other people, its policy on industrial research. In the past few weeks, there has been a spate of conflicting views. The general principle is that industry must stand on its own feet, but it also seems to be accepted that the government should provide industries with better incentives to help themselves by means of contracts for research and development. In principle, in other words, the government appears to be committed to a reduction of direct support for industrial research and reduction of its own intramural applied research. The difficulty, of course, is that the present government is as hard pressed as all its predecessors to know how to make radical reductions in the scale of activity at the public laboratories. The result is that, because of these conflicting pressures, there is a danger that the government may be making less than full use of its own scientific resources, and that its parsimony towards industry may be a false economy. Certainly there is no doubt that in its support of long term research, especially in fields such as telecommunications, the present policy may be shortsighted.

100 Years Ago



SCIENCE LECTURES FOR THE PEOPLE

IT is the great weakness of Science in this country that its professors are rather a mass of incoherent units than an organised body eager to influence others and themselves enjoying the privileges of such influence.

Each one is apt to work too much by himself, and while he often exhibits the most rare skill in discovering truth, he too frequently leaves to others less able than he the task of bringing his labours before the world at large.

Now, while the man of science complains with much justice that his pursuits have not been recognised by the rulers of our country, he ought not to forget that it is likewise his duty to help others, in doing which he will help himself. Whatever be the faults of our rulers, they are eminently sensitive to public opinion; men of science, therefore, have only to prove to the people that they are a useful class in order to have their services recognised. It is really absurd to suppose that one of the most intelligent and useful bodies of men in this country could not obtain their just demands if they set themselves earnestly and unitedly to the task. They have hitherto tried to prove to our rulers that the promotion of science will benefit the country, but have met with only indifferent success; let them supplement their endeavours by convincing our rulers that to promote it will be for their own benefit, and they are sure to succeed. Success, in fine, will not be attained by a policy of isolation, but by leavening the whole mass of the community with the love of science, and when this is done science will rise to its just place in the councils of the nation.

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OLD WORLD

SELECT COMMITTEE

Means to an End

THE House of Commons Select Committee on Science and Technology this week heard the Department of Trade and Industry give an account of itself. Mr David Price, Parliamentary Under-secretary and a former member of the select committee, described a department looking critically at most aspects of its services to British industry and technology.

Much of the interrogation was, inevitably, directed to Concorde and nuclear reactor development, which together took more than 58 per cent of the department's funds in 1970-71. The Concorde project cost £67.6 million, and although most of the developmental work has been completed, Mr Price explained that tooling costs are rising, and for some years ahead there will be airworthiness tests to be paid for as well as stress and fatigue tests.

Will worthy aerospace projects be endangered if Concorde accounts for 68 per cent of the department's aerospace budget in 1970-71? Mr Price shrugged off this suggestion from Dr Tom Stuttaford. He implied that funds will be available for projects that show themselves to be worthy of support. Last year, civil aero-engine projects other than Concorde (including the Trident 3B and the RB211) were supported to the tune of £7.4 million and the civil share of Ministry of Defence aviation research claimed £15.6 million.

Internal policy reviews at present incomplete allowed Mr Price to avoid laying down a firm line on nuclear reactor development finance, but he let it be known that the government has no intention of letting research in reactor development go to private industry. There was a case, however, for

a greater proportion of the cost of exploitation to be borne by industry or by the Central Electricity Generating Board. In 1970-71, £34.0 million was spent on reactor development, the lion's share (£22.7 million) going to research on the fast breeder reactor.

These major items of expenditure apart, the committee was chiefly interested in how the new department is forming policy. Does, in fact, the flame for reform and innovation which led to Mr Wedgwood Benn's green paper still burn bright? Mr Price emphasized his functional approach to research and development, saying that internal reviews are being conducted in this light. He was adamant that there is no room in the budget for research without prospect of industrial application. There is a clear need that research organizations should be closely associated with the ministry which they can best serve, and an early indication of this pragmatic approach was the transfer of four former Mintech research units, the Fire Research Station, the Forest Products Research Laboratory, the Hydraulics Research Station and the Water Pollution Research Laboratory, to the Department of the Environment. When the internal reviews are completed, said Mr Price, it may be found necessary to make changes in the administration of other units.

Research in the department's own establishments cost £12.9 million in 1970-71. These establishments include the National Physical Laboratory (£6.5 million), the National Engineering Laboratory (£2.9 million), the Warren Spring Laboratory (£1.2 million) and the Torrey Research Station (£0.5 million). Dr I. Maddock, controller of Industrial Technology, answering on behalf of the DTI, agreed that the directors of the various research institutes should and did have a degree of latitude in the research their organizations pursued, but that overall control rested with the department.

But if the department's own research organizations are favoured, it is perfectly clear that the industrial research associations are very much the poor relation. Limited to sharing out only £4 million annually, in 1970-71 they were awarded only £3.25 million and there is every indication that things will get tougher. The memorandum submitted by Mr Price to the committee states that "the terms on which grants are made are being stiffened whenever they come up for renewal, and the department is reviewing its policies for the RAs with the object of establishing priorities and determining which of them justify continued public support".

Clearly, the one hope of survival for the associations is contract research, but what future for them is there in this area? In industrial recessions, many

firms make economies by farming out their research work, and the contract research firms then profit. The boom in contract research has been under way for over ten years now, but even so the Fulmer Research Institute increased its turnover last year by 20 per cent to £360,000. The Huntingdon Research Centre showed a rise in turnover of about 30 per cent, much of its business coming from America or the continent. In contrast, the British Coal Utilization Research Association will be wound up later this summer as the Coal Board withdraws its support. Likewise, the British Non-Ferrous Metals Research Association reports an operating deficit for 1970 of £36,618, and to balance its budget the association has been forced to raise subscription fees by 25 per cent. The secret of success in contract research, amply demonstrated by the Fulmer unit, seems to be diversity of research facility, and freedom from the obligation to serve the needs of any one industry—especially a nationalized industry.

Unless the weakly RAs adopt such an approach they will surely succumb, for it seems certain that whatever policy changes are contemplated by the DTI on completing its internal reviews, succour for non-profit-making research will not be among them.

ACCELERATORS

Towards the Attainable

from our Soviet Correspondent

PARTICLES with energies of 2 GeV have now been obtained on the new colliding beam electron-positron accelerator of the Novosibirsk Institute of Nuclear Physics of the Academy of Sciences of the USSR. The device is similar in type to that at Frascati, but the energies obtained at Novosibirsk are already almost twice as great as those obtained with the Italian machine. They are four times higher than those so far achieved in French experiments. The Novosibirsk accelerator is designed to operate at 3.5 GeV, and it is expected that this energy level will actually be attained by the end of this year. The Novosibirsk research team says that at 2 GeV, it is possible to generate all known elementary particles and their anti-particles. When the accelerator is operating at the intended energy level, the planners say they hope to identify and study previously unknown particles.

UK Research and Development Expenditure (£ million) 1970-71

Department of Trade and Industry

Civil AEA	48.0
Civil aerospace	99.0
Research establishments	13.0
Other	14.0

Other Government Departments

Research councils and universities	97.0
Defence	228.0
Other governmental R&D	43.0

Non-governmental R&D

Public corporations	46.0
Private industry	383.0
Overseas finance	30.0

MALNUTRITION

Beans Means Meat

from our Special Correspondent

Portmeirion, Merionethshire

"PEOPLE do not ingest nutrients, they eat food." This was the premise underlining the unique Unorthodox Foods Conference held in the sumptuous surroundings of the Portmeirion Hotel here last week. The conference, organized by 77-year-old Mrs Amabel Williams-Ellis (her husband is the distinguished architect and the builder of the village at Portmeirion), was attended by more than a hundred guests including nutritionists, representatives from overseas aid organizations, industrialists and domestic science teachers. They had in common the hope of lessening the world's food problems.

Mrs Williams-Ellis says that on a recent visit to Africa, she was moved by the sight of children suffering from kwashiorkor, and that she has since learned that the problem can be eased significantly by the availability of new and unconventional sources of protein. One example is fish protein concentrate, a product that can be made from the 40 per cent of the world catch which never reaches the table and which costs as little as £0.06 per pound of protein compared with £2.70 per pound of protein in the form of white fish. Leaf protein, which can be extracted from most kinds of foliage, is another foodstuff that could be made available cheaply to developing countries. The machinery required to extract enough of this product to supplement usefully the diet of 80,000 people could be installed for a mere £3,000. The conference also heard from Mr W. R. McDowall, a representative of British Petroleum, whose plant at Grangemouth in Scotland is producing annually 4,000 tons of a microorganism which feeds on the otherwise useless wax fraction making up one per cent of crude oil. The general conclusion seems to have been that no single source of protein can meet the demand, but that much can be done by pooling innovations.

With such a potential, why are 15 million people dying every year from malnutrition? "Acceptability is the keyword," said Mrs Williams-Ellis. "We are not sensitive enough to the basic anthropology of the situation." The prejudices and taboos even of starving people are difficult to overcome and any aid programme cannot expect that novel foods will be accepted at first offering. Food habits can and do change, however. This was the view put forward by Professor F. Aylward, head of the Department of Food Science at the University of Reading, and he instanced as an example the rise in consumption of soya protein in the United States during the past five years.

Nutritionists believe that if the people in developing countries are to alter their food habits, they will do so not because they are convinced of their nutrient value, but because they like them. One encouragement is to give the food "prestige value", which persuaded the conference last week that because the new foods rarely grace the tables of the aid-givers, the recipients regard what they are given as the very opposite of prestigious. "It is essential to remove the stigma of poor man's food," said Mr N. W. Pirie, head of the biochemistry department at Rothamsted Experimental Station and chairman of the conference, and Mrs Williams-Ellis set about doing just this in a way as unorthodox as the foodstuffs she was championing. Where better to seed the rumour that "all the best Europeans are eating it" than the dining rooms of a luxury hotel renowned for its *haute cuisine*?

Throughout the day, conference members were treated to dishes liberally fortified with delicacies such as A 3/5, Itona TVP and FPC. Using techniques pioneered by members of the Cauldron Club (a group of local ladies organized by Mrs Williams-Ellis on her return from Africa) the hotel chef demonstrated just how acceptably these unconventional products can be incorporated into conventional European dishes. He was assisted by chef Leonard, on loan for the day from the Lord Rank Research Centre, who convincingly demonstrated that fish mousse and liver pâté, both made from a high-protein fungal microorganism grown on starch, are as good as the real thing and certainly more nutritious; compared with his fungal meat loaf, beef steak, with its mere 15 per cent of protein, seemed positively frugal.

Beans, making a very good stab at being meat in the form of spun protein chunks (courtesy of Courtaulds), were served as a very passable goulash at dinner, and supplemented the main course of roast eland. This large antelope is seen by many as the "aid on the doorstep" answer to some of the problems in Africa. At £0.05 per pound, low in saturated fat content and delicious, it might well be.

More a get-together than a conference, the occasion none the less succeeded in several of its objectives. It demonstrated that there was a real basis for Professor Aylward's prophecy that these unorthodox foods will be in common use in 25 years' time. But more than that, it brought together a wide range of people with the interests of the undernourished at heart, and whether their concern was nutritional, sociological, financial or merely humanitarian, they left with minds that were at least as enlightened as their stomachs.

MARINE CONSERVATION

Lundy Comes First

THE proposal first put forward last winter for making the underwater littoral round the island of Lundy in the Bristol Channel a marine nature reserve dedicated to the study and protection of a distinctive flora and fauna and for informed diving recreation has been making steady progress this spring. The scheme was originally put forward by a group of marine biologists with West Country connexions and coordinated by Mr Keith Hiscock of the Menai Bridge Marine Science Laboratories. It has now been backed by the Nature Conservancy, the Lundy Field Society and the Devon Trust for Nature Conservation.

With the purchase of the island by the National Trust (for which the Landmark Commission acts as administrator) the moment seemed appropriate. Lundy has a unique situation in that it shares the semi-tropical regime of the south-west peninsula, but due to its prolonged isolation (it is 11 miles from shore and 20 miles from a port) has avoided the depredations of Victorian collectors which decimated the natural populations of the seashore of the remainder of the region. It remains rich in species and indeed has a number of flora and fauna unknown elsewhere in the British Isles. The Landmark Commission has now endorsed the scheme and some funds have been made available for a fortnight's expedition by a range of scientists this summer to draw up a biological catalogue and to make recommendations in the light of field experience of how a marine nature reserve round the island should be run. Mr Hiscock is coordinating the field expedition which is intended to be multidisciplinary.

The Landmark Commission, which administers the island and its facilities on behalf of the owners, the National Trust, is in any case considering introducing sea-wardens and starting a small museum, so the coordination of these can be effected with the interests of the marine nature reserve in mind from the beginning. The Landmark Commission, having adopted the proposal, is being kept in close touch with the further enquiries and thinking being put into the plan by the scientists. The immediate objectives of the proposal are to protect the natural populations of marine flora and fauna, to provide facilities for tourist divers to encourage an informed interest in natural history and to enable the public to glimpse the underwater world through special displays. The promoters are taking the scheme slowly and systematically. They wish to avoid such pitfalls—mostly sins of omission—that some other schemes abroad have encountered.

NEW WORLD

PSAC Hiccoughs Over 2,4,5-T

by our Washington Correspondent

IN October 1969, the President's science adviser, Dr Lee Dubridge, set up a panel to study the widely used herbicide 2,4,5-T and its suspected teratogenic properties. The panel duly reviewed the available evidence but, as is customary with reports compiled for the President's Science Advisory Committee, its findings were not made public. The report of the PSAC panel would probably be of little more than historical interest had it not recently been discovered and issued this month as a model of what such a report should be. "This review represents an outstanding case study of government regulation of pesticide," states a press release issued by the Office of Science and Technology. In a preface to the report Edward E. David, who succeeded Dubridge as science adviser and chairman of PSAC last August, says that the report is "an excellent case study" and that the principles it brings out "will be useful in dealing with other pesticides, and indeed with chemicals of all kinds." How far does the review live up to the claims made for it?

Some people who have seen the report call it excellent, others say it contains glaring and serious omissions. The verdict may vary according to the perspective of the beholder and the date and purpose for which the review was compiled. Members of the PSAC panel say it was written so long ago—about March or April 1970—that they can remember little about it. One member believes that, to begin with, the only real data available were the study conducted by the Bionetics Corporation which first showed that commercial preparations of 2,4,5-T could produce malformations in the offspring of rats.

Although the PSAC panel was told to concentrate on the domestic use of the herbicide, this member recalls there was the feeling that evaluation of the military use of 2,4,5-T as a defoliant in Vietnam was the real impetus for undertaking the study. Dubridge, in the first public reference to the Bionetics study in October 1969, said that the Defense Department in its defoliation operations over Vietnam "will restrict the use of 2,4,5-T to areas remote from the population", but for another six months he was unable to stop either the Defense Department or the Department of Agriculture from exposing the populations under their respective control to 2,4,5-T. It was not until April 1970, shortly after the

PSAC panel apparently completed its report and confirmed that the Bionetics results could not be dismissed, that the Pentagon's long attachment to 2,4,5-T was broken. On April 15 last year the Surgeon General announced that 2,4,5-T would be suspended for use over water, around the home and on food crops, enabling the Defense Department to declare at the same time that the military use of the herbicide would fall in line with domestic use.

If the purpose of the PSAC panel on 2,4,5-T was simply to strengthen Dubridge's hand by providing an authoritative review of the then available evidence, the panel members seem to have performed a thorough and careful task. But their report, which does not seem to have changed significantly since they completed it, is being presented by the Office of Science and Technology as an outstanding case study of government regulation, which for all intents and purposes covers the evidence on 2,4,5-T up until March 1971, the date of David's preface.

Judged in this light, the PSAC report has several curious features. One is the inclusion of a section on the importance of 2,4,5-T as a defoliating agent. This is an issue which might properly be regarded as beyond the competence of a scientific review panel; if it were to be considered, an impartial presentation of evidence might be expected. The PSAC report is not impartial—it quotes at length, though admittedly without endorsement, a series of anecdotes told by an Admiral W. E. Lemos indicating that defoliation and crop destruction by herbicides are militarily effective. A staff member of the OST has said that this was the only sort of evidence available. In fact there exist several studies made in or for the Department of Defense that challenge the effectiveness of defoliation and crop destruction programmes, including, for example, a survey compiled by the Rand Corporation in 1967 suggesting that only 5 per cent of enemy soldiers depended on locally grown crops.

Why did the PSAC panel fail to cite these reports or to present the evidence objectively? "The reason PSAC did it was a sheer tribute to the political power wielded by the Joint Chiefs of Staff," says one observer. "It was not the habit of PSAC to buck the Joint Chiefs of Staff, at least not under Dubridge." Whatever the reason, the abandonment of scientific objectivity in

a kow-tow to the military is an awkward adornment of what purports to be an "outstanding case study of government regulation of pesticide", since if partisan evidence from the Defense Department can be quoted without challenge how is the reader to be sure that similar courtesies have not been extended to the manufacturers of a pesticide and their customers?

Another anxiety raised by the PSAC study lies in its treatment of the economic consequences of 2,4,5-T. The weighing of economic benefits against the possible health hazards of a pesticide is of course a crucial question for regulatory authorities and one that an "outstanding case study" might well be expected to address. But although the panel attempts to estimate the costs of substituting alternative herbicides to 2,4,5-T for a variety of uses, the report contains no suggestions as to how these should be weighed against the toxicological risks.

This failure to grapple with an important issue is in a way not surprising since the necessary expertise in social science is lacking. None of the members of the PSAC panel seems to be an economist, there are only three social scientists on PSAC, and the Office of Science and Technology has yet to appoint its first social scientist staff member. This composition of course reflects the long-standing dominance of physical scientists on PSAC.

Even when on home territory, the PSAC panel report does not seem to have covered all the ground. "For what it does say, it is a reasonably good report, but there are two important recommendations that should have been made but were not," says Matthew S. Meselson, professor of biology at Harvard University. Meselson believes the report should have been more circumspect in its consideration of the dose-response relationship between 2,4,5-T and the terata it induces, and that more attention should have been devoted to the possibility that dioxin, the highly lethal contaminant of 2,4,5-T preparations, may accumulate in body tissues. In essence, the PSAC panel report says that there exists a minimum dosage below which the herbicide will not cause damage, so that animals and humans may safely be exposed to concentrations of 2,4,5-T that are smaller by a safety factor of 10 to 100 than this minimum or threshold dosage.

Meselson agrees that the idea of a threshold is plausible, though unproved, but he believes the PSAC panel report should have pointed out the crucial nature of this assumption, and that in its recommendations for further research it should explicitly have cited the need for establishing a dose-response curve for the teratogenicity of 2,4,5-T. The consequences of the response being linear would be profound.

Meselson also considers the report should have mentioned the possibility that the dioxin contaminant of 2,4,5-T may accumulate in the body fat, as might be expected from its chemical structure. Edward J. Burger, an OST staff member of the panel, says this issue was discussed in panel meetings but not in the report since it was just one among several unproved possibilities. But the possibility that an extraordinarily toxic contaminant of a widely used herbicide (the LD₅₀ of dioxin is 0.0006 parts per million for female guinea-pigs) may be sufficiently stable in the environment and soluble in fat and liver to enter food chains and the human diet does seem worth mentioning in what is claimed to be a model report. Moreover, the PSAC panel report offers no scientific justification for its recommendation that the level of dioxin in 2,4,5-T should be limited to not more than 0.5 ppm and should be reduced to 0.1 ppm. This is good news for manufacturers because, as the PSAC panel report points out, present technology allows 2,4,5-T to be manufactured with less than 1 ppm of dioxin.

But the smallest quantity of lethal impurity a manufacturer is capable of admitting into his product is not necessarily the quantity guaranteed to be harmless to man and beast. A very rough calculation indicates that even the 0.1 ppm limit urged by the PSAC panel report raises questions. Animals are quite often found to contain in their bodies concentrations of DDT of the order of 10 ppm; if the pesticide were not DDT but a solution of 2,4,5-T containing 0.1 ppm of dioxin, the animal would contain 10⁻¹² parts of dioxin, which is only a thousand times less than the LD₅₀ for guinea-pigs (2,4,5-T does not accumulate, but dioxin may). The PSAC panel report includes no calculations of this kind, much less any discussion of their significance.

Of wider interest than the issue of 2,4,5-T, the PSAC panel recommends that new information on a pesticide "should be disseminated promptly to individuals and organizations that are legitimately concerned". The original experiments on 2,4,5-T by the Bionetics Corporation, under contract to the National Cancer Institute, were com-

pleted about August 1968, and it was not until 14 months later, after extensive prodding by individuals inside and outside the government, that the existence of the report was first made known by Dubridge.

The PSAC panel report does not deem it worth describing this extraordinary chain of events in any detail. Another recommendation of general importance—now embodied in an administration bill before the Congress—is that a legal mechanism should be established for restricting a pesticide temporarily on the basis of new information that indicates it may be hazardous. Of the two mechanisms available at present, cancellation is too mild—the manufacturer can continue to sell the substance pending an appeal—and suspension is too drastic, since a compound can only be suspended if shown to be an imminent hazard, and once suspended is unlikely ever to return to the market.

A third recommendation is a level-headed warning on the interpretation of teratogenesis. Most chemicals might be shown to be teratogenic in animals, the PSAC report says, and it is not possible to extrapolate the results of animal experiments directly to man. "To restrict or ban usage of chemicals on the basis of demonstration of teratogenicity at dose levels which far exceed actual or expected exposures is unreasonable and could well deny the usage of chemicals whose benefits far outweigh the risks."

The President's Science Advisory Committee won its spurs as a body of physical scientists, concerned almost exclusively with defence matters, whose particular *forte* was the ability to take on issues such as strategic weapons that cut across different parts of the government. In some ways PSAC is less important than it was since Congress is beginning to take an interest in the questioning and analysis of defence issues which was formerly done by PSAC or not at all. The panel report on 2,4,5-T suggests that PSAC, in trying to extend its sphere of authority to the civilian issues more usually covered by the National Academy of Sciences, has yet to acquire the tact and urge for completeness with which the academy seeks to hallmark its public reports.

DEFENCE SPENDING

FAS Flays Foster

by our Washington Correspondent

"MANY new occurrences inevitably come with spring," Senator Stuart Symington told the Senate in March this year, "some pleasant—warmer weather, Japanese cherry blossoms; some unpleasant—proposed higher defence budget, warnings of grave new

dangers to this country because of new developments in Soviet weaponry . . . In reasonably good humour I say to my colleagues and the taxpayer, 'Here we go again'". Within a few days of Senator Symington's observation, John S. Foster, the director of defence research and engineering in the Department of Defense, launched his springtime offensive against Congress on behalf of the \$7,880 million the Pentagon is requesting for research and development in the coming financial year. The two prongs of his attack were that the Soviet Union is now spending the equivalent of \$3,000 million a year more than the United States in these areas and that, as a result, "the Soviet Union could assume technological superiority in military research and development in the latter half of this decade".



John S. Foster, director of defence research and engineering. (From *The Washingtonian*.) (By John Lewis, ©1971 *Washingtonian Magazine*.)

The Senate Armed Services committee, before which Foster delivered this testimony, may well have been impressed not only by the bad news but also by Foster's magnificent forbearance in not demanding an extra \$3,000 million for himself. Last week, in an unprecedented open hearing, the ad hoc subcommittee on research and development of the Senate Armed Services committee had the chance of hearing Foster's claims dissected and dismissed by the Federation of American Scientists. Pentagon statements about Soviet research, the federation said in a 50-page report entitled *Is there a Research Gap?*, have amounted to a "classical numbers game featuring selective disclosure, questionable assumptions, exaggeratedly precise estimates, misleading language and alarming non-sequitur conclusions".

The federation, a small but energetic lobbying group, devotes most of its energy to issues of arms control and disarmament. Its membership, composed almost entirely of scientists, includes several former members of the President's Science Advisory Committee. The analysis of the Defense Department's new claims about Soviet military research spending was put together by a federation committee which included Professor Marvin L. Goldberger of Princeton University, a phy-

sicist and a former member of PSAC, and George W. Rathjens, professor of political science at MIT.

The federation was represented before the Senate subcommittee last week by Rathjens and by Jeremy J. Stone, director of the FAS. Rathjens and Stone told the subcommittee that the federation was not calling for cuts in the military research budget but was concerned only to puncture the alarmist statements on Soviet research spending put out by Foster and the Defense Department. The basis of Foster's claims that the USSR is now outspending the United States in military research is his statement that the growth in spending that formerly characterized the Soviet space effort has been transferred to military research. But the FAS quotes Foster as telling the Senate research and development subcommittee a year ago that "it is very unfortunate we cannot separate the Soviet's space effort from their atomic energy effort and from their defense effort".

How has the Pentagon suddenly been able to make the separation? How in any case can it estimate Soviet military research spending each year since 1960 with such precision? Dr E. Rechlin, Foster's assistant in the department of defence research and engineering, has claimed that a graph of Soviet military research spending prepared by the department is accurate "to within 10 per cent or so", yet similar graphs produced on separate occasions by Rechlin and Foster differ by 14 per cent.

The FAS statement also points out that dollar spending on military research and development is a poor index of technological preparedness; development funds account for a far greater proportion than research, yet development costs, in the Soviet case, may represent the wasteful Soviet policy of developing to completion every weapon that can be built, including many that are duplicative or ultimately deficient.

Asked if there was a scientific gap between the American and Soviet scientific research efforts, Rathjens told the Senate subcommittee that "in my judgment they are doing quite well in maths, certain areas of physics, and hydrodynamics. But in computer sciences, and in biology and chemistry, particularly biology, they are far behind". Rathjens was more concerned at the credibility gap in the United States as exemplified by Foster's misrepresentations of the Soviet research capability. "Some of the best minds being produced in this country will not be available to the government simply because they will have become conditioned to not believing what the government tells them, and because they will be profoundly sceptical of the purposes to which their inventions and ingenuity might be put," Rathjens said.

Short Notes

Impurification

DURING the present vogue for applied research that rules in Washington, basic scientists are finding it profitable to emphasize the practical consequences, however indirect, that flow from their activities. Nobody has been smarter at this than the astronomers, who are now advertising telescopes as valuable instruments for the detection of air pollutants. The National Science Foundation parted only this month (May 20) with a \$113,000 grant for the perusal of air pollution through telescopes. But astronomers are not alone in finding ingenious applications for pure research. The importance of botanical systematics to the national defence effort, for example, was probably realized by only a few until pointed out by a committee of taxonomists in a recent report prepared for the Federal Council for Science and Technology. A little known example from World War II, the taxonomists remarked, would serve to demonstrate "the usefulness that . . . taxonomic information may have in times of national peril. During one period in the war, Japanese balloons carrying fire bombs wreaked havoc in the forests of the American northwest. Eventually a balloon was recovered with several bags of ballast sand attached. The sand contained a large number of micro-organisms, which, after careful identification and interpretation, was found to represent both cold and warm water forms. Their overlap in geographic range was limited to a small area off one of the mainland islands of Japan. Subsequent bombing of this beach area destroyed the balloon launching sites."

Keeping Up with Cancer

THE American Heart Association is determined not to be stolen a march upon by its rival, the American Cancer Society. When the panel of consultants to the Senate health subcommittee called for the National Cancer Institute to be broken off from the National Institutes of Health and established as a separate agency, the Heart Association demanded similar treatment for the National Heart and Lung Institute. Earlier this month President Nixon countered the Senate panel proposal by raising the National Cancer Institute to the status of a bureau. Naturally the American Heart Association has now asked for bureau status for the heart institute as well, even though elevation is devoid of practical significance.

Jackson Lab Chides NSF

AMONG the many educational institutions which have been abandoned by the National Science Foundation in its recent budgets, few can lodge so graphic a complaint as the Jackson Laboratory of Bar Harbor in Maine. Out of its archives the laboratory has dusted off a group photograph of participants in one of its precollege summer training programmes which, until recently, were supported by the NSF. The photo, of the class of 1945, includes two students who 25 years later were to make their name for the same, but independent, finding, the discovery of reverse transcriptases—Howard Temin in the second row and David Baltimore in the third. The NSF's support of the training programme, the laboratory director tartly observes, "seems to have been a pretty good investment—I can't help wondering if our country can really find a better one".

Sex Too

LINUS PAULING, the only male winner of two Nobel prizes, added a new dimension to his scientific reputation last year when he advocated vitamin C as a defence against the common cold, chiefly on the evidence of his personal experience and that of his wife. Asked at a recent meeting of the Commonwealth Club of California whether large doses of vitamin C depressed the libido, Pauling replied he didn't think so—"in fact, I think it has the opposite effect on myself". Colds, sex—maybe the miraculous ascorbate will also be recommended for helping a man shine at chemistry, or peacemaking, or carrying off Nobel prizes.

NEWS AND VIEWS

Early Man in the East Rudolf Basin

EAST AFRICA has long been recognized as one of the best sources of early man material. Dr Louis Leakey's persistent faith in Olduvai Gorge in Tanzania has been clearly justified in the past two decades, and now field work by his son Richard Leakey and his colleagues in the Lake Rudolf area, north of Olduvai, is providing discoveries which may well be equally significant in the study of human origins; for example, no less than sixteen specimens of hominid and a rich collection of vertebrate remains were recovered by the 1970 expedition to the area (see page 241 of this issue). The deposits to the east of Lake Rudolf in Kenya, and in the Omo River Valley to the north, in Ethiopia, extend back in time from those at Olduvai and thus add another two million years to the history of human evolution. Although the oldest levels at Olduvai are dated at just under two million years, the oldest at Omo are probably just under four million years, so that at sites in East Africa can be seen evidence of the various stages of human evolution—the older levels have the remains of the australopithecines and the younger levels have, in succession, early hominines and, finally, fully sapient types.

The fossil hominid material recovered by both the 1969 and 1970 seasons in the east Rudolf area is significant for several reasons. The chief interest is Richard Leakey's suggestion that the genus *Homo* is represented there in deposits which are possibly more than one million years old. Although the evidence for *Homo* at Koobi Fora and Ileret is certainly not indisputable, the fragmentary mandible (KNM-ER-730) does seem to present a complex of characters not found in other hominids known from this period of time. It may even be the best evidence so far of the genus before the Pleistocene hominids from Java. The newly discovered limb bones, so far unanalysed, may also be of considerable importance, because although several femoral ends have been attributed to the australopithecines no remains of the shaft have previously been found. If KNM-ER-736 and 738 can confidently be assigned to that genus then they will indeed be unique and can be expected to add considerably to knowledge of that group. Moreover, the limb bones and the locomotory patterns that they reflect form a component of the total trait complex of the form represented and thus may contribute significant discriminating parameters for taxonomic purposes. The third important result of field work in the Lake Rudolf area is the reiteration of the contemporaneity of the "robust" and "gracile" australopithecine types and the morphological stability of these types through a long period of time. Sites are already known outside the Rudolf area which show the robust and gracile types coexisting apparently contemporaneously, for example, Bed I at Olduvai and Swartkrans in South Africa. Furthermore, the site at Sterkfontein, near Swartkrans, shows a wide range of size differences but so far without clear evidence of the larger form.

Richard Leakey's suggestion that the size and morphological differences between the two types relate to sexual dimorphism is not a new idea, but it has not so far been widely accepted. The theory is attractive to some workers because it would avoid certain problems of competitive

exclusion, sympatry and morphological intergrades, all of which would cease to be taxonomically troubling if sexual dimorphism were acceptable. But how then to explain why some sites contain predominantly one type only? Some workers consider it unlikely that just one sex, if the australopithecines were indeed widely sexually dimorphic, would be preserved at these sites; the continuing field work, however, has not entirely supported this point of view. Although modern man is not highly sexually dimorphic, some of the other ground living higher primates are; gorillas, chimps and baboons are the obvious examples.

In contrast with the single species idea is the view that at least two taxonomic groups are indicated by the heterogeneous australopithecine remains. In the view of some workers this polymorphism corresponds to generic distinctions; to others it represents specific or sub-specific groupings. Dr John Robinson's "dietary hypothesis" (*Nature*, **205**, 121; 1965) states that dental differences between the robust and gracile types reflect differences in diet, and consequently in behaviour, with the robust type being predominantly herbivorous and the smaller type omnivorous. This explanation has found recent support in the work of Dr Clifford Jolly (*Man*, **5**, 5; 1970), who has defined a "graminivorous" (seed-eating) dentition in some baboons and points out certain similarities between this and the dentition of the robust australopithecines. The dietary hypothesis has the advantage of solving the problem of sympatry because in this way the two types could coexist. The very long period of coexistence of the two types of australopithecines would then indicate that they were not in serious competition for the same resources; either they were members of the same population or they made different demands on the environment.

The taxonomy of the australopithecines is one of the most hotly disputed topics in palaeoanthropology today. It is possible, however, that the new discoveries of limb bones at east Rudolf may be useful in moderating this controversy and at the same time aid in the definition of valid taxonomic criteria which are now lacking. If the limb bones from the two types can be shown to have different locomotory patterns, then more than one taxonomic category is likely to be represented. Equally, similarity of the locomotory pattern could be held as evidence of one species because two hominid lines, already similar in many respects and with the same locomotory adaptation, would suggest an extraordinary parallelism.

Leakey's suggestion that the genera *Homo* and *Australopithecus* were evolving along parallel lines in East Africa might find some support in the recently described robust australopithecine from Chesowanja (*Nature*, **230**, 509; 1971). Although the authors have described this specimen as less specialized and possibly more adaptable than previously known robust types, a completely objective analysis is difficult because of its poor state of preservation. It must be pointed out, however, that such parallelism between closely related genera, if it indeed existed, would have been unlikely to continue locally for a long period because of competition.

CYCLIC AMP

Kith and Kinase

from our Molecular Biology Correspondent

FOR those persons who regret the passing of vitalism, the nearest thing to the *élan vital* is nowadays cyclic AMP. It has been implicated in the control of an extraordinary variety of biological processes—most recently in visual excitation, where it is reported to encompass a 'change in' membrane permeability, following capture of a photon in a retinal rod (Bitensky, Gorman and Miller, *Proc. US Nat. Acad. Sci.*, **68**, 561; 1971).

The search for a cyclic AMP receptor protein in a number of systems has now turned up what may well prove to be a universally relevant mechanism. Gill and Garren (*ibid.*, 786), for example, have isolated such a receptor from adrenal cortex cells, and find that it is associated with protein phosphokinase activity. This latter has for some time been known to be activated by the cyclic nucleotide. When the crude product is chromatographed on an ion-exchange column, the two activities superimpose in the first peak, but thereafter barely separated peaks of acceptor and kinase activity are eluted. The first component also migrates electrophoretically as a single zone; but when cyclic AMP is added, it dissociates to give a fully active kinase that migrates independently, and is no longer affected by the AMP. This effect is substantially reversible: when the dissociated kinase and receptor are mixed in the absence of the nucleotide, they associate, with suppression of kinase activity. From an estimate of the molecular weights of the complex and its constituents a 1:1 stoichiometry is inferred. But the last peak eluted from the column, which is associated with high kinase and low acceptor activity (the residuum of the latter being eluted separately), seems to generate kinase dimers on dissociation.

In the same issue of the *Proceedings*, Erlichman, Hirsch and Rosen (*ibid.*, 731) describe the isolation of a cyclic AMP-activated kinase from beef heart. In this case gel filtration or ion exchange chromatography gave a major and a minor peak of activity. When cyclic AMP was present the kinase activity peak was displaced to a position corresponding to lower molecular weight, and when recovered was no longer subject to control by the nucleotide. Again the dissociation was reversible: in the absence of cyclic AMP, the kinase and the residual fraction, which possessed binding activity, recombined, and the kinase once more became AMP-sensitive. A precisely similar situation was uncovered by Krebs's group (Reimann *et al.*, *Biochem.*

Biophys. Res. Commun., **42**, 187; 1971), working with rabbit skeletal muscle. Thus it is supposed that the system has the function of allowing the cyclic AMP concentration to control the kinase activity, which in turn governs the activity of a variety of enzymes. This mechanism was foreseen by Tao, Salas and Lipmann in an article published last year, and offers a new instance of an association between catalytic and regulatory subunits, of which the archetypal example is perhaps Gerhart's aspartate transcarbamylase.

The part played by phosphorylation with kinase in glycogen metabolism in muscle is the subject of the latest instalments in the great epic of glycogenolysis that is being unfolded by Krebs and his associates. Walsh *et al.* (*J. Biol. Chem.*, **246**, 1968; 1971) have shown that, in a word, the protein kinase phosphorylates phosphorylase kinase, which phosphorylates phosphorylase *b* to give phosphorylase *a*. Any readers who have lost the thread of the narrative may

wish to be reminded that phosphorylase kinase, which operates on muscle phosphorylase, is strongly activated on being itself phosphorylated. This process is promoted by cyclic AMP, but a second and slower activation mechanism exists, which is independent of cyclic AMP, and is shown to involve autophosphorylation of the phosphorylase kinase (an enzyme that requires ATP, but not cyclic AMP). Walsh *et al.* (*ibid.*, 1977) have also discovered at specific protein inhibitor that associates with the cyclic AMP-dependent kinase, but does not bind the nucleotide. Isolation and enzymatic characterization of the kinase are described by Reimann *et al.* (*ibid.*, 1986). Two components with similar activity are present, as in the kinases from other tissues. They offer the suggestion, based on the low substrate specificity, that the same enzyme may be responsible for phosphorylating all the many proteins in different tissues, the activity of which is controlled by this means.

New Lower Limit for Quasar Masses

IT now seems firmly established that the mass of a quasi-stellar object must exceed 10^7 solar masses. In the next issue of *Nature Physical Science*, D. P. Whitmire and C. N. Davids describe yet another method of estimating this important parameter. Their method is based on the observation that in many QSOs with both emission and absorption lines, the absorption redshifts are greater than the emission redshifts, presumably because the absorption lines arise in material previously ejected from, but now falling back upon, the QSO. The ejected material evidently failed to reach escape velocity, so the relative velocity of the shell and QSO, which is given by the difference in the absorption and emission redshifts, permits an estimate of the minimum mass interior to the shell, provided that the shell radius is known. Their estimate of $2 \times 10^7 M_{\odot}$ for the mass of the quasar PHL 5200 is of particular interest because they have calculated the minimum shell radius directly from a change in the absorption and emission redshifts which seems to have taken place over a period of one year; the estimated mass limit is found to be independent of the source distance.

No assumption is made about the cosmological nature of QSOs, and only the difference in the emission and absorption redshifts is regarded as necessarily a genuine velocity shift. Their limit of $2 \times 10^7 M_{\odot}$ agrees with earlier estimates based on the assumption that QSOs lie at cosmological distances and is in particularly good agree-

ment with those estimates based on general energy considerations which give lower limits of 10^8 and $10^7 M_{\odot}$, depending on whether the energy source is nuclear or gravitational.

The lower limit to a QSO mass of $\sim 10^7 M_{\odot}$ based on optical observations will be very much an underestimate for the total mass associated with a QSO if, for instance, the optical activity represents activity in the nucleus of a galaxy. The similarity of the structure of radio sources associated with QSOs and with radio galaxies suggests that this may indeed be the case and it would therefore be of interest to have a reliable upper mass limit. The methods which have been proposed and which seem capable of yielding a useful upper limit are all based on finding a QSO in a cluster of galaxies and comparing its redshift with that of the associated cluster members. Finding genuine QSOs in clusters of galaxies does not, however, seem to be the easiest of occupations, and an estimate of $M \leq 5 \times 10^{13} M_{\odot}$ by Bachall and Salpeter (*Astrophys. J. Lett.*, **158**, L15; 1970), based on the object B264, was criticized by Barnothy (*Astrophys. J. Lett.*, **159**, L133; 1970) and is now known to be irrelevant because B264 has now been shown to be not a QSO but a compact galaxy. Nevertheless, at least some QSOs may occur in clusters and this in itself would seem to imply a limit of 10^{13} to $10^{14} M_{\odot}$, for it seems unlikely that the mass of a cluster member exceeds the mass of a typical cluster.

PHOTOSYNTHESIS

Clue from Mutant Alga

from our Photosynthesis Correspondent

THERE is every indication that mutant strains of algae may prove to be powerful tools for studying the reactions which occur on the water-splitting, oxidizing side of photosystem two (S2). These reactions are among the most poorly characterized of all the events that take place during photosynthesis by algae and higher plants. It is this part of the photosynthetic apparatus which extracts electrons from water resulting in the production of molecular oxygen and the release of protons. The electrons are then raised to a reducing potential capable of combining the protons with carbon dioxide to produce carbohydrate. The energy for this process is, of course, obtained from light which is probably absorbed by two photosystems S2 and S1 acting in series (see D. A. Walker, *Nature*, **226**, 1204; 1970).

Most workers in photosynthesis would now agree that manganese and chloride ions act as cofactors for the S2 oxidizing reactions, but their mode of action is far from certain. Extensive studies from the laboratories of Kok (B. Kok, B. Forbush and N. McGloin, *Photochem. Photobiol.*, **11**, 457; 1970) and Joliot (G. Barbieri, R. Delosme and P. Joliot, *Photochem. Photobiol.*, **12**, 197; 1970) have indicated that a multivalent component is oxidized by S2 light but its chemical nature is not known. Although information about this part of the electron transport chain is flimsy at present, an encouraging development which will undoubtedly extend what is known of the primary reactions involved in oxygen evolution is outlined in an article from Levine's laboratory (B. L. Epel and R. P. Levine, *Biochim. Biophys. Acta*, **226**, 154; 1971).

Epel and Levine have successfully isolated a mutant strain of the unicellular alga *Chlamydomonas reinhardtii* that has lost the capacity to carry out reactions on the oxidizing side of S2. The mutant cells were unable to evolve oxygen although their chlorophyll content falls within the range generally found for the wild type. They also had a normal ratio of chlorophyll *a* to *b*. Although chloroplasts isolated from the mutant showed little or no Hill activity, they were able to carry out S1 reactions such as the photoreduction of NADP using a 2,6-dichlorophenolindophenol-ascorbate couple. As would be expected the mutant showed no time dependent changes in the yield of chlorophyll fluorescence. Of course, normal photosynthetic systems show changes in chlorophyll fluorescence as the number of open reaction centre traps varies

with time. Epel and Levine did show, however, that a p-hydroquinone-ascorbate electron donor system could restore the fluorescence yield changes, confirming Yamashita and Butler's contention (*Plant Physiol.*, **44**, 435; 1969) that this donor system provides electrons to the oxidizing side of S2.

Perhaps the most surprising feature of this mutant was the weakness of its chlorophyll fluorescence as compared with wild-type cells. This was an unexpected observation because it is well known that blocking electron flow on the reducing side of the S2 traps usually results in an increase of fluorescence yield. This increase in fluorescence can be simply explained in terms of a closing of the S2 traps which would also

be predicted for a similar blocking on the oxidizing side of S2. To explain the apparent contradiction between experimental findings and theoretical expectation, Epel and Levine suggest that a rapid back reaction occurs between the primary reductant of S2 and the oxidizing S2 reaction centre. In this case the chlorophyll fluorescence is kept low because the S2 traps are maintained in a state capable of accepting absorbed light energy in the form of an excited electron.

So far the preliminary studies on this mutant have not added much new information about the reactions associated with the photo-oxidation of water, but they do open up many possible lines of research.

Controlled Transcription *in vitro*

NEXT Wednesday's issue of *Nature New Biology* carries the story of one of those *tour de force* experiments with which molecular biologists periodically dazzle their expectant audiences. De Crombrughe and six colleagues, a modestly sized group by the standards of the day and by the experiments, have reconstructed a cell free system which includes all the enzymes, regulator molecules and activators required for the regulated transcription of the genes of the lactose (lac) operon of *Escherichia coli*. Their experiments indicate that neither more nor less than seven components are necessary and sufficient for the initiation, elongation and termination of lac operon RNA molecules and the repression of this set of genes.

As a source of the lac operon DNA, de Crombrughe *et al.* have used the DNA of a hybrid temperate bacteriophage which carries the entire *E. coli* lac operon. This phage DNA provides them not only with a suitable template but also allows them to exploit a neat hybridization method for measuring the amount of lac operon RNA transcribed. Apart from the DNA template, the system requires *E. coli* RNA polymerase, complete with its sigma factor, together with another specificity factor called cyclic AMP receptor protein (CRP) and cyclic AMP itself. The so-called rho factor is needed to accurately terminate RNA synthesis, and to switch the process on and off an appropriate inducer molecule and lac repressor protein have to be added to the system.

How do all these various components function in a coordinated manner? From the results of their own experiments and the vast assemblage of information about the lac operon which has been accumulated over the years,

de Crombrughe *et al.* paint the following picture. The lac repressor protein sits on the lac DNA blocking the initiation of transcription; in this state the genes are switched off. When, however, an inducer molecule such as a galactoside is added, it homes onto the repressor, binds to it and inactivates it. Once active repressor has been effectively removed from the system, the cyclic AMP binding protein associates with cyclic AMP and then tightly binds to some sequence at the beginning of the lac DNA. The formation of this complex between the lac DNA and the cyclic AMP protein is, they believe, a prerequisite for the attachment of *E. coli* RNA polymerase, complete with sigma factor, to the promoter site of the operon.

Once the polymerase has attached to the DNA and initiated the synthesis of lac RNA, the enzyme can proceed to transcribe the entire operon. When it reaches the end of the genetic message, the rho factor somehow causes the polymerase to terminate RNA synthesis and cast off from the DNA template releasing its RNA transcript. The only other molecule which may play a part in this process is the guanosine tetraphosphate ppGpp which significantly inhibits transcription by the purified, complete cell-free system. The mechanism of this inhibition is unknown.

The experiments of de Crombrughe *et al.* define all the essential components required for the controlled transcription of this operon. Their system seems to be complete and it leaves no room for further, so far undiscovered, elements. It provides the most detailed explanation so far available of how the expression of a set of related genes can be controlled.

GEOMAGNETIC REVERSALS

Long Term Pattern?

from our Geomagnetism Correspondent

BEFORE the discovery of geomagnetic field reversals and the elucidation of the late Cainozoic polarity-time scale, the periodicities of known field variations ranged from seconds (external field effects) through centuries (secular variations of the non-dipole field generally in the range 10^2 to 10^3 years) to tens of thousands of years (dipole variations). The discovery in the 1950s and 1960s of variations having periods of millions or even tens of millions of years was, for obvious reasons, unexpected; though the improvement in both the quantity and quality of palaeomagnetic data has amply confirmed their reality since.

But do even longer periodicities exist? There would seem to be no *a priori* reasons why not, though in view of the wide time coverage of data required for the successful detection of such variations the difficulties involved in the search are obvious. McElhinny (*Science*, **172**, 157; 1971), however, has now collected together and analysed all the palaeomagnetic directional data for the Phanerozoic (actually 1,094 investigations) and finds that there do indeed seem to be field variations having periodicities of hundreds of millions of years.

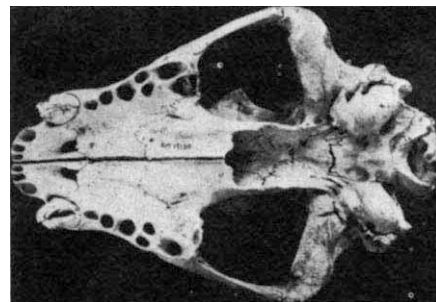
The quantity that McElhinny has found significant is the relative proportions of normally and reversely magnetized rocks within the various geological epochs. During the Upper Tertiary, normal and reversed rocks apparently came upon the scene in equal proportions—the classic fraction which is used, among other things, as evidence for field reversal as opposed to self-reversal. In the Lower Tertiary there was a slight preponderance of reversed (55 per cent) over normal (45 per cent). The Upper Cretaceous, on the other hand, saw a violent lurch from normality (about 75 per cent) which had apparently lasted since the Permo-Triassic. Between the Silurian and Permian in one direction and the Silurian and Cambrian in the other the field was apparently more reversed than normal, although around the Silurian itself there was a 50–50 interval. From the Middle Carboniferous to the Lower Permian there was a period when hardly any normally magnetized rocks emerged at all—presumably a reflexion of the long Kiaman magnetic interval. In summary, the Earth's magnetic field was predominantly normal during the Mesozoic and predominantly reversed during the Palaeozoic. The "periodicity" of this variation (only one "period" can be seen, of course) is thus about 350–400 million years.

According to McElhinny, the pro-

portion of mixed results (palaeomagnetic results involving both normal and reversed polarities) within any given geological epoch may be taken as a measure of reversal frequency—the greater the reversal frequency the more likely a palaeomagnetic data set is to contain both polarities. From plotting the mixed polarity proportion as a function of time throughout the Phanerozoic it then becomes clear that reversals were rare during the Upper Carboniferous and Permian (the long reversed Kiaman interval) and during the Upper Ordovician and Silurian. The greatest reversal frequencies, on the other hand, seem to have occurred during the Upper Cambrian and Lower Ordovician, the Lower and Middle Devonian and the Upper Tertiary—the third of these having been amply confirmed by sea floor spreading data.

Perhaps a surprising point to emerge from the analysis, however, is that of the 1,094 Phanerozoic palaeomagnetic studies 44 per cent represent normal rocks and 56 per cent represent reversed rocks. If the Tertiary is excluded these figures become 43 per cent and 57 per cent, respectively. At this level of analysis therefore the commonly stated view, supposedly based on experimental data, that rock polarities taken altogether are evenly divided seems to be invalidated.

MAMMALIA

Cape Lion Skull

THE Cape lion, *Panthera leo melanochaita*, is known only by its name and a few bones preserved in museums around the world; of these specimens only three are skulls. One of these is now briefly described by J. Meester of the Mammal Research Unit at the University of Pretoria (*Ann. Transv. Mus.*, **27**, 27; 1971). It was dug up in the early 1960s at Betty's Bay on the southern Cape coast and was later presented to the Kaffrarian Museum in King William's Town. It lacks both its lower jaw and all its upper teeth except the canines, but it shares its distinctive broad muzzle—the feature which distinguishes it from living lions—with a skull recovered earlier from Murraysburg in the Cape.

Transformation and Autoagglutination

WITH so much attention being focused these days on the changes in the surfaces of cells after they have been transformed to malignancy by tumour viruses and other agents, any new parameter, such as that reported in next week's *Nature New Biology* by Edwards, Campbell and Williams, which reveals differences between the surfaces of transformed and untransformed cells is sure of a warm welcome.

Edwards and his colleagues have discovered an extremely simple method for distinguishing untransformed cells of the BHK line from these cells transformed by polyoma virus. When suspensions of untransformed BHK cells, produced by freeing cells from the glass or plastic dishes in which they are grown by exposure to small amounts of trypsin, are gently swirled in Hanks solution (a defined mineral medium), they adhere to one another and they can be measured easily with a Coulter particle counter. By contrast, when BHK cells transformed by polyoma virus are treated in an identical manner little or no aggregation can be detected.

After repeated serial passages, cloned

untransformed and transformed BHK cells maintain this different pattern of autoagglutination, and only by allowing transformed cells to grow into very dense cultures did Edwards *et al.* find it possible to increase the extent to which they adhere to each other. This pattern of aggregation is, of course, precisely the reverse of the cell agglutination brought about by plant agglutinins such as Concanavalin A and wheat germ agglutinin; transformed cells are more susceptible than untransformed to agglutination by these agents.

It will be interesting to learn whether the pattern of autoagglutination shown by BHK cells and their polyoma transformants is common to other cell lines and to cells transformed by other viruses. It will be equally interesting to see whether and how all the various surface changes following transformation, which can be detected by a variety of methods, are interrelated. In the meantime, Edwards *et al.* have provided another test, remarkable for its simplicity, for the transformed cell phenotype, and the trait they assay, cell adhesion, may be of importance to malignancy *in vivo*.

COLICINS

Killer Proteins in Action

from our Cell Biology Correspondent

LIKE an outsider coming from behind the field and forcing the favourite to a photo finish and a dead heat, Senior and Holland report, in the latest issue of the *Proceedings of the US National Academy of Sciences* (68, 959; 1971), that colicin E3 acts by activating a specific ribonuclease in sensitive *Escherichia coli* which cleaves a fragment from the 3' terminal end of the 16S RNA of the 30S ribosomal subunit and so inactivates the ribosome. In the same issue of the *Proceedings*, indeed in the very next paper (*ibid.*, 964), Bowman, Dahlberg, Ikemura, Konisky and Nomura report identical conclusions.

When not otherwise occupied in reconstructing the large and small ribosomal subunits of *E. coli* from their constituent nucleic acids and proteins, Nomura has spent much time deciphering the biology of colicins, in particular colicin E3. It was he and his co-workers who showed that these protein molecules, which are specified by extra-chromosomal genetic elements in certain strains of *E. coli*, can adsorb to the surfaces of susceptible *E. coli* and, without penetrating the surface, rapidly inhibit protein synthesis without at the same time affecting nucleic acid metabolism. Moreover, it was Nomura who proved that ribosomes isolated from susceptible *E. coli*, after exposure to colicins, are less able to support polypeptide synthesis *in vitro* than ribosomes from untreated cells and that this defect resides in the small 30S ribosomal subunit.

The mystery of these observations, which the two groups have now solved, was simply the question of how a protein which only binds to the outside surface of a bacterium can cause the specific inactivation of the 30S ribosomes of that cell. The only plausible hypothesis was that by binding to the bacterium's surface the colicin causes some conformational change in the cell membrane. The inactivation of 30S ribosomes might then ensue if the conformational change activates either a ribonuclease or a protease which could modify and inactivate the ribosomal RNA or some crucial ribosomal protein.

Pursuing this line of argument, Bowman *et al.* isolated the proteins and RNA from 30S ribosomes of bacteria exposed to colicin E3 and from untreated bacteria. These were then used in a series of reciprocal reconstructions of the 30S ribosome and the reconstructed particles were tested for their ability to support protein synthesis. Such a mammoth task, which only

Nomura's group is equipped to do, proved that the defect in the 30S ribosomes of cells exposed to colicin E3 resides in the 16S ribosomal RNA and not in the complement of ribosomal proteins. Bowman *et al.* therefore made fingerprints of the 16S ribosomal RNA from cells treated with colicin E3 and compared them with fingerprints of 16S RNA from untreated cells. There were several oligonucleotides missing from the former RNA, including the oligonucleotide which contains the normal 3' terminal base of 16S ribosomal RNA.

Not satisfied at leaving things at that stage, Nomura's group successfully isolated a fragment of about fifty bases from cells exposed to colicin E3. On fingerprinting, this fragment was found to yield the 3' terminal and other missing oligonucleotides. It is the piece of RNA cleaved from the 16S ribosomal RNA by the specific ribonuclease

which must be activated when colicin E3 binds to the cell surface.

Although not able to do the elegant ribosome reconstruction experiments of Nomura and his colleagues, Senior and Holland analysed by gel electrophoresis the 16S RNA and proteins of the 30S ribosomes from cells exposed and not exposed to colicin E3. The only differences they could detect were with the 16S RNA. Further analysis of this RNA from cells exposed to colicin E3 showed, in fact, that it sediments at 15S, not 16S, and fingerprints show that this 15S RNA lacks the 3' terminal sequence of intact 16S RNA.

All that now remains to complete this quite remarkable story of how colicin E3 works is the identification of the location and nature of the ribonuclease, which is activated when this colicin binds to cells and which is so specific for some sequence in 16S ribosomal RNA.

Magnetic Fields in Radio Sources

THE intense signals from most galactic and extragalactic radio sources are thought to be produced by the synchrotron process in which relativistic electrons gyrate in the source magnetic fields. Although the magnetic field structure has been derived from many sources from measurements of linear polarization, it has proved difficult to obtain reliable estimates of the magnitude of these fields. Arguments based on an assumed equipartition between the energy of relativistic particles and magnetic fields suggest an order of magnitude for the fields of 10^{-4} gauss. A more direct assessment of the field strength can be obtained from investigations of the synchrotron self-absorption in a source, but this requires measurements of the radio flux over a wide frequency range as well as interferometer observations, usually on intercontinental baselines.

A simple and potentially powerful technique for measuring source magnetic fields is now emerging based on a determination of the percentage circular polarization. Several years ago, theoretical calculations by Sciama and Rees (*Nature*, 216, 147; 1967) and by Legg and Westfold (*Astrophys. J.*, 154, 499; 1968) showed that the synchrotron process would give weak but significant circular polarization which present-day experimental techniques can detect. Observers now agree that the circular polarization of about one per cent which is emitted by the planet Jupiter indicates the presence of a field of 0.4 gauss in its van Allen belts. In extra-

galactic sources of high emissivity, Conway, Gilbert, Raimond and Weiler (*Mon. Not. Roy. Astron. Soc.*, 152, 1p; 1971) believe there is significant circular polarization of a few tenths of a per cent which indicates fields of up to 10^{-3} gauss.

In next Monday's *Nature Physical Science*, E. R. Seaquist of the David Dunlap Observatory, Ontario, Canada, reports a detailed study of the $z=1.187$ quasi-stellar object PKS 1127-14 made with the 46-metre telescope of the Algonquin Radio Observatory. He finds significant circular polarization which varies on a time scale of 1.5 years and has a maximum value of one per cent. Synchrotron emission theory indicates that the magnetic field in PKS 1127-14 would be about 6×10^{-2} gauss. The importance of this result is that it can be checked against the many other observations of this source over a wide range of wavelengths which can be used to build up a good picture of its physical properties. Measurements of the variability of PKS 1127-14 show that its small diameter (≤ 0.003 seconds of arc) component also has a time scale of 1.5 years. This same component has a maximum in its spectrum at 7,000 MHz which suggests a magnetic field strength comparable with that indicated by the circular polarization measurements. The results look very promising and should stimulate more extensive observations and calculations of the circular polarization from more realistic magnetic configurations of radio sources.

ENZYMES

A Crying Inhibitor

from our Cell Biology Correspondent

THERE is clearly more to tears than meets the eye, for, as Kueppers has just reported (*Biochim. Biophys. Acta*, **229**, 845; 1971), human tears contain an effective inhibitor of the proteolytic enzymes trypsin and chymotrypsin. Motivated by the discovery of inhibitors of trypsin in human urine and pancreatic juice and at least five distinct substances in human serum which inhibit trypsin, chymotrypsin and elastase, Kueppers apparently cast around for another body fluid to test, and hit upon tears. The snag that human tears are, in the adult world, obtainable in only small amounts was overcome with liberal whiffs of ammonia and a sensitivity assay for proteolytic activity, namely the spectroscopic measurement of nitroaniline liberated by trypsin from $N\alpha$ -benzoyl-DL-arginine- p -nitroanilide-HCl.

After giving his subjects whiffs of ammonia, Kueppers took 0.02–0.05 ml. of tears from each weeping eye and tested it against 1 μ g of trypsin in 1 ml. of a 1 per cent aqueous solution of substrate. 1 ml. of these induced tears inhibited between 10 μ g and 35 μ g of trypsin and the unstimulated lachrymal fluid collected from two subjects had enough inhibitor to block 43 μ g and 25 μ g of trypsin.

Using thin layer chromatography, Kueppers was able to show that the inhibitor in tears differs from the previously characterized protease inhibitors found in various other human body fluids. But with a weight in the range 3,000 to 9,000 daltons, the chromatographic and electrophoretic mobilities of the human tear inhibitor recall those of an inhibitor of proteases found in bovine body fluids. The human factor is, however, more susceptible to heat inactivation than its bovine counterpart. As to the physiological role of this substance, it can only be surmised that it may be in tears to protect the cornea and conjunctiva.

IONOPHORES

Peptide Isolated

from our Membrane Correspondent

AN ionophore is a compound which is capable of reacting with a simple ion to give a complex which shows a preferential partition from water into a hydrophobic phase. Ionophores thus effect the translocation of the complexed ions across natural and artificial lipid membranes. Most of the ionophores so far described are produced by microorganisms or fungi and until recently none had been demonstrated

as endogenous products in higher organisms.

Ionophores are given an important role in D. E. Green's model of active transport across the mitochondrial membrane, and last year Blondin and Green postulated that ionophores were endogenous to mitochondria of beef heart. Blondin, DeCastro and Senior (*Biochem. Biophys. Res. Commun.*, **43**, 28; 1971) have now reported the isolation and partial characterization of just such an ionophore. Because the ionophore valinomycin produces effects in mitochondria similar to the postulated properties of the endogenous ionophore, Blondin *et al.* were able to work out a successful chromatographic procedure for isolating the endogenous ionophore by assuming that its chromatographic behaviour would be similar to that of valinomycin. By again using valinomycin as a model they were able to assay the endogenous ionophore by its stimulation of potassium ion accumulation from solutions of low potassium acetate concentration into energized mitochondria. Because of its very low concentration in mitochondria, they were unable to isolate enough material to completely characterize the new ionophore, but their results indicate that it is a cyclic

peptide with an approximate molecular weight of 1100 which complexes sodium and potassium to about the same degree and they tentatively suggest the structure (Pro. Leu. Val. Ser.)₃.

This successful isolation of an endogenous ionophore from mitochondria should stimulate a search for ionophores in other mammalian cell membranes. Perhaps the discovery of a calcium-specific ionophore in the sarcoplasmic reticulum can be anticipated, and Hyman (*Fed. Proc.*, **30**, 314; 1971) has presented some rather inconclusive evidence which suggests that an ionophore may exist in erythrocyte membranes. There are grounds for thinking, however, that the mitochondrial membrane has a unique structure from which generalizations regarding other membranes are largely guesswork. If this view is right, the ionophore search party is likely to return empty handed.

ICE CAPS

Selenium Pollution?

from our Geomagnetism Correspondent

FROM the purely scientific point of view the relative permanence of the major ice caps is one of their most endearing properties. For much of the ice which

Strong Pulses and the Crab Pulsar

It was, of course, the occasional unusually strong radio pulses emitted by one of the stars in the Crab Nebula that identified it as a pulsar even before its periodicity was recognized. A special study of the strong pulses is reported in next Monday's *Nature Physical Science* by Carl Heiles and John M. Rankin, based on observations at Arecibo Observatory, Puerto Rico. Their work refers to low radio frequencies where the contribution of the nebula tends to swamp all but the stronger pulsar pulses in any case. Although Heiles and Rankin do not speculate on the origin of the pulses, their work shows that the strong pulses have other unusual features which will require a special explanation.

The work reported by Heiles and Rankin was done basically in two stages. In one experiment the strong pulses were recorded simultaneously in two channels at 318 MHz and 111 MHz, and in the second experiment at 111 MHz and 74 MHz. Data obtained at 430 MHz by the same observers were also available.

On the face of it, the most striking feature of the observations is that strong pulses at 318 MHz do not usually correspond to particularly strong pulses at 111 MHz, and *vice versa*. In other

words, the spectral index varies quite unusually. This immediately contrasts with all other pulsars where there is no case when pulse energies at two frequencies are not correlated. Comparison between pulses at 111 MHz and 74 MHz could not be expressed so definitely by Heiles and Rankin because only six strong pulses were detected at the lower frequency during the observations. In general, however, they remark that at each frequency it is possible to detect between twenty and forty strong pulses in 2½ hours of observation. In other words, they are writing about a type of pulse which occurs about once every five minutes on average. And they point out that interstellar scintillations probably have a negligible effect on the results.

So Heiles and Rankin use their data to produce some expressions for the frequency dependence of the strong pulses. It is this part of their article which will be quoted in the future theoretical work which one day will account for the occurrence of the strong pulses. At present, however, the immediate problem is to explain the evidence for the variability of the spectral index of the strong pulses. Values between zero and three, and possibly even greater variations, can occur.

formed perhaps many thousands of years ago still exists and lies buried beneath subsequent annual layers, each of which is in some way typical of the age in which it was produced. Each layer will, for example, have sampled the atmospheric fallout appropriate to the time of its formation. Thus if the ice can be dated—as it often can by firm stratigraphy, oxygen isotope analysis or lead geochronology, for example—and its contents analysed, it is often possible to determine the pollution content of the atmosphere over periods of time.

This technique is particularly useful in determining the effects of man's activities upon the content of the atmosphere. It is well known, for example, that the burning of fossil fuels—oil, coal, lignite and natural gas—results in the transfer of various chemical elements and compounds from the continents to the atmosphere where they circulate and from which they ultimately fall. The analysis of dated ice samples thus means that today's atmospheric pollution can be compared with that of the recent past or even with the "standard" atmosphere before the industrial revolution. As a result, it is now known that the quantity of sulphur entering the atmosphere as a consequence of fossil fuel combustion is about the same as that arising from natural causes such as volcanic activity and the oxidation of organic matter.

Although sulphur may be the most severe of the polluting chemicals, what of other lesser elements? Selenium, for example, is emitted during the burning of fossil fuels and is, moreover, a member of the same group of elements as sulphur in the periodic table. So does it behave in a similar way to sulphur once it enters the atmosphere? To answer this question, Weiss *et al.* (*Science*, **172**, 261; 1971) have analysed ice samples of fourteen different ages ranging from 800 BC to the summer of 1965 for both sulphur and selenium—and have found that the proportions of each element which finally fall upon the ice sheets of Greenland and Antarctica are different. In pre-1960 ice samples, for example, the Se/S ratios vary between 2.9 and 5.9×10^{-4} , whereas in post-1960 samples the corresponding range is 1.1 to 1.6×10^{-4} . The 1960 sample itself seems to be transitional with an Se/S ratio of 2.2×10^{-4} . Thus although the combustion of fossil fuels has significantly increased the sulphur in the atmosphere in recent years, the selenium from the same fuels has not entered the atmospheric circulation to anything like the same extent. This assumes, of course, that much of the sulphur and selenium in the ice really has originated from the solid Earth, though this seems to be a warranted assumption. According to one study, Se/S ratios in petroleum range between

0.31 and 1.22×10^{-4} and the average ratio for coal is 2.9×10^{-4} . Moreover, most crustal rocks have ratios of the order of 10^{-4} . In sea water, on the other hand, this ratio is usually three orders of magnitude smaller.

The conclusion to be drawn from all this is that the selenium from the burning of fossil fuel has not yet travelled over large distances, a situation which, when compared to the known behaviour of sulphur, is tantamount to saying that it hardly circulates about the atmosphere at all. Why should this be so? Weiss and his colleagues put it down to the chemical behaviour of the relevant tetravalent oxides. Sulphur forms the relatively stable gas SO_2 upon oxidation whereas selenium forms SeO_2 , a white solid which is readily converted to red elemental Se at the sight of anything remotely resembling a reducing agent. On the other hand, biological processes at low temperatures result in volatile compounds of both sulphur and selenium. Selenium, for example, is volatilized from land plants as organic compounds of, as yet, unknown composition. Nevertheless, the proportion

of Se in these forms is much smaller than that from fuels; and so most of it never reaches the colder regions of the Earth.

ENVIRONMENT

Remote Sensors at Work

from a Correspondent

AT the seventh international symposium on remote sensing of environment held at the University of Michigan from May 17 to 21, the topics for discussion included instrumentation, data handling, automated photo interpretation and the application of remote sensing to geology, soil science, ecology, oceanography, meteorology, land planning and pollution monitoring.

Inevitably, considerable attention was directed to the Earth Resource Technology Satellite Programme (ERTS (A)) which is to be launched in March 1972 and several speakers outlined their own country's proposed participation in this programme. American workers re-

Transcriptional Control in Transformed Cells

THE precise state of the genome of the tumour virus in the transformed cell is still a mystery. In the cells that have been examined there are a few copies of the viral genome per cell (Gelb *et al.*, *J. Mol. Biol.*, **57**, 129; 1971), which are probably integrated into the host's chromosomes, and Lindberg and Darnell (*Proc. US Nat. Acad. Sci.*, **65**, 1089; 1970) have shown that, in some cases at least, the SV40-specific mRNA molecules are considerably larger than those expected of a single-polycistronic transcript of the viral genome.

At the Lepetit Symposium in Paris last November, Darnell reported that these large mRNA molecules contained host-specific sequences as well as virus-specific sequences. This seemed to confirm the chromosomal location of the viral genome and suggested that the control of transcription of the integrated viral genome might differ from that of the autonomously replicating SV40 during a lytic infection.

This suspicion will now be strengthened further by the report of Sauer in next Wednesday's *Nature New Biology*. He has examined the SV40-specific mRNAs synthesized, in the presence of cytosine arabinoside, in an SV40-transformed monkey cell and a lytically infected monkey cell. Cytosine arabinoside is known to inhibit DNA synthesis and subsequent viral functions in the lytically infected cell and to

restrict the synthesis of some mRNA molecules: the species of mRNA made in the presence of the inhibitor being defined as early mRNA. Sauer has found that in SV40 transformed cells treated with inhibitor, not only is early mRNA made but some late SV40 mRNA is also synthesized, in amounts which correspond to the transcription of about 10 per cent of the SV40 genome.

This surprising observation suggests that the physical state of the integrated viral genome has altered the nature of the sites at which viral RNA is initiated. This might occur by the simple act of removing the superhelical turns (if they exist inside the cell) or perhaps by fusing an otherwise untranscribed viral gene into a cell gene which is transcribed in the presence of cytosine arabinoside.

Sauer also observed that during a period of 18 months, the proportion of the virus-specific mRNA sequences which are transcribed in the SV40 transformed cell and which are homologous to SV40 late mRNA fell by about a half. Before the implications of these interesting observations are fully understood, much more information is needed about the nature of the integration of the tumour viral genome and the dependence of the initiation of transcription on the physical structure of the DNA template.

ported results of experiments with multispectral scanners and the identification of various terrain features, including rocks, soils, crops and types of vegetation. Several speakers described some of the factors affecting the spectral signature and methods of recognizing and minimizing the effect of these factors.

The section on instrumentation covered both passive and active sensors. Drs Yu. I. Rabinovich, G. G. Shchukin and V. V. Melentyev (The Institute of Space Methods for Earth Sciences, Institute of Physics, Leningrad) described the determination of the water content of clouds and surface water temperatures using a microwave radiometer mounted in an IL-18 aircraft and operating at wavelengths of 0.80, 1.35, 1.60, 3.2, and 8.5 cm; by comparing microwave and infrared measurements, it is possible to estimate errors in the determination of water temperature.

Drs L. Silva, R. Hoffer and J. Cipra (Purdue University) described a new extended wavelength field spectroradiometer. This instrument uses four detectors (Si, PbS, InSb and HgCdTe) and covers the spectrum from 0.37 μm to 14.2 μm ; scan time is adjustable from 0.5 to 30 seconds. Reliable spectral measurements in the field, as distinct from the laboratory measurements of small samples, can now be made. Several speakers described new uses for radar; Dr E. L. Frost (US Army Electronics Command, Fort Monmouth, New Jersey) reported the monitoring of mosquito movements over short distances. "Dot angels" observed by the radar operator are compared with truck and helicopter trappings and the correlation is good.

Various methods for the monitoring of oil slicks, including radar, photography and the multispectral scanner, were reported. Drs J. C. Munday and M. F. Penney (Virginia Institute of Marine Science, Gloucester Point) reported that various oils at thicknesses less than 1 μm are easily imaged on film and that film density varies with the thickness; good slick detection is provided by colour infrared film.

There are now several instruments and methods for analysing film imagery as well as conventional densitometric methods. Drs E. Ranz and S. Schneider (Institut für Landeskunde, Bad Godesberg, Germany) described the use of the black-and-white equidensity film 'Agfacontour' for the identification of lineaments and other geological features.

The symposium was characterized by enthusiasm for and optimism about the newer techniques of remote sensing and it seemed that there was insufficient awareness that they are tools to supplement and enhance but not necessarily replace existing methods of scientific investigation.

TASTE

Sweet Sensitivity

from our Membrane Correspondent
BECAUSE the funding of research in olfaction and taste is almost nonexistent, it remains an area of science where a little work generates a large amount of speculation. Dastoli's work on the isolation, from bovine tongue epithelium, of proteins which apparently showed a specific binding of sweet substances led to the attractive theory that these proteins were "receptor sites" involved in the mechanism of response of the taste buds. Those physiologists who did not find Dastoli's evidence very convincing will have their prejudices strengthened by a recent report by Kayama and Kurihara (*J. Gen. Physiol.*, **57**, 297; 1971).

By carefully dissecting out the papillae and carrying out polyacrylamide gel electrophoresis on protein extracts from different parts of the tongue epithelium, these workers have shown that Dastoli's "sweet-sensitive" protein is found not only in the papillae but also to an equal extent in the surrounding epithelium which is devoid of taste buds. Furthermore, they could detect no protein band which was found only in the papillae extracts. These results indicate that the receptor site, if indeed a protein, is present in relatively small amounts, probably as some firmly bound membrane constituent. Alternatively, if Dastoli's protein is still to be considered as the receptor site, an explanation is needed as to why it is present in such large quantities where it has no receptor function.

Cooperation in the Immune Response

COOPERATION in the immune response is presently the vogue word among cellular immunologists. Semantic variants are "helper" effects and synergism. In all instances it is envisaged that at least two cells are involved in the production of humoral antibodies of various kinds.

The first real indications of cooperation emerged from some experiments of Claman and his colleagues (Claman, H. N., and Chaperon, E. A., *Transpl. Rev.*, **1**, 92; 1969), which showed that neither thymocytes nor bone marrow cells injected into irradiated mice along with sheep red blood cells were capable of initiating a substantial immune response. When the two cell populations were combined the immune response far exceeded the sum of the responses of the individual cell populations. Since these experiments it has become fashionable to regard the potentiated response as between B and T cells which are broadly to be thought of, in mice, as of bone marrow and thymic origin respectively; the cell from the bone marrow is supposed to produce the antibody and the T cell is thought to help in some unspecified manner.

The best guess about the function of the T cell has emerged from immunologists at the National Institute for Medical Research, London, who have, almost collectively, suggested that the T cell is an antigen-handling device. Now the story is beginning to acquire complexity, part of which may be real, part based on false assumptions. The problem is whether T and B cells are heterogeneous. This heterogeneity could be in a variety of ways; each category could include cells of differing degrees of differentional maturity; the B and T cells found in different

places in the lymphoid system (that is, lymph nodes and spleen) could be cell populations of which the behavioural pattern may in some way reflect their immediate anatomic origins; but, overall, either or both B and T cells may have heterogeneity in relation to the specificity of the receptor molecules they are supposed to carry. An article by Playfair and Purves in the next issue of *Nature New Biology* hints at some of these possibilities.

Playfair and Purves have found that in Claman-type cooperation experiments in which the number of spleen or bone marrow cells (both used as a source of B cells) has been varied independently but always with a standard large number of thymocytes (as a source of T cells) the curves for cell number/antibody response for the two cell species are not the same. They suggest that the B cells in spleen and bone marrow are different—more specifically there may be two kinds of B cells, B1 and B2, which differ in the ratios in which they are to be found in spleen and bone marrow. B1 cells are said to be less exacting in relation to their requirement for cooperation than are B2. The number of B1 cells in spleen and marrow is said to be roughly equal but the spleen contains more B2 cells than the marrow.

The interpretation offered for these ingenious experiments ignores the possibility that the effects seen could result from the existence of two different kinds of T cells which might also be of differing frequency in spleen and marrow. If B1, B2 and T1 and T2 have to be accommodated as four lymphocyte conditions before receptor specificities, are considered, then indeed cellular immunologists are in for a bewildering time.

SURVEY OF EUROPEAN SCIENCE

Policy for European Science

IN the past few days it has become apparent that the United Kingdom, together with Denmark, Eire and Norway, will soon belong to the European Economic Community. To be sure, there are a great many difficulties to be surmounted before the euphoria generated last week by Mr Edward Heath's visit to Paris is translated into reality, but at least there is more hope now that the community will be enlarged than there has been for the best part of a decade. Limited though the scope may be of the Treaty of Rome as it is at present understood, nobody should be surprised that the issue of membership—or non-membership—has belatedly assumed a quite exceptional importance in the calculations of British politicians and their electors. This is a measure of the extent to which it is now apparent to all concerned that much more than mere economics—important though economic prosperity may be—is involved in the proposed enlargement of the EEC. So much is clear from a simple consideration of how the proposed accession of the United Kingdom and its associates would transform the character of the European technical community.

The pages which follow show, again this year, how vigorous yet how disjointed and dispersed are the scientific activities of European nations. Everybody knows how Davy and Faraday managed to go jaunting on the mainland when the Napoleonic Wars were at their height, yet it is still painfully apparent that the intellectual community of Europe remains a collection of separate parts rather than an integrated whole. Organizations such as CERN have done splendid work in showing how the informal ways of the nineteenth century can be translated into the idiom of the present, when much scientific research depends on the existence of huge machinery, and when the careers of young people depend on how quickly they can be seen to make some kind of mark, yet the fact that these collaborative supranational organizations are necessary is by itself a proof that natural forms of collaboration are not easily arranged. Although the EEC as such makes no formal provision for science, technology and education, the belief that an economic community must in the end be a cultural community as well is one of the strongest arguments for hoping that the proposals for enlarging the EEC should be supported.

How realistic is this hope? And how can the United Kingdom and its associates help to improve on the steps now being taken towards integration on the mainland of Europe? These are more interesting questions than the largely economic calculations that have so far dominated the issue of the enlargement of the community, although it is only fair to acknowledge that the Treaty of Rome does contain seeds from which in the end are bound to spring cultural developments of the kind likely in the end to put their stamp on the character of European life. How can a glorified customs union bring about such important changes? The immediate goal, and the immediate attraction for countries such as Britain, is that the disappearance of tariff barriers will bring (after whatever transition period may be agreed) access to a larger market. The other side of this strictly economic coin,

of course, is that the harsh winds of competition will blow more fiercely, which is why there are already anxious inquiries in Britain into such imponderable questions as the long-term viability of the automobile industry, of shipbuilding, even of heavy electrical engineering. For most purposes, however, it is a sufficiently good first approximation to suppose that mere membership of the EEC will bring in its train only comparatively gradual changes. If one industry should disappear, some other will grow up to take its place—and in particular to employ the people who would otherwise be out of work. This, however, is only a short-term calculation, for a decade or two. Further ahead, it is inevitable that the economic ups and downs within the EEC can only be ironed out by a vigorous application of those clauses in the Treaty of Rome which provide for the free movement of people and of investment. At that stage, still some way off even for the community as it exists at present, the customs union will be at least as accessible to all who live in it as, say, the United States. Formal steps towards political integration within the EEC are not necessary for this purpose, although they would be, of course, important and relevant.

The advantages that might flow from such developments are easy to identify and potentially rewarding, even if the EEC remains no larger than at present. Industrially, for example, the hope is that there will be a progressive transformation of the pattern of industry that will make it possible for individual communities to concentrate on what they do well. Too much energy is at present spent, in all European countries, on keeping up with what are thought to be the Joneses. No single economic unit is able cheerfully to ignore the challenge to develop electronic computers, or equipment for telecommunications. The duplication of plans for the development of fast reactors for the nuclear power industry is a great trap into which too many governments have fallen. In too many important and expensive fields European nations remain disinclined to rely on their neighbours for essential services. The result is not merely the waste of technical resources necessary to bring independent projects to fruition or what passes for fruition, but also a pointless fragmentation of the supposedly unified market for many of the projects of technology. The best hope is that this is a passing phase.

The benefits in fundamental science are in the long

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run potentially even more important. In aggregate, Europe is a generous provider not merely of places in universities and other institutions at which young men and women can be technically trained but also of the funds for financing individual research projects. Here again, however, as with the distribution of the funds available for industrial development, resources are too uniformly spread. Already within Britain it is clear that not all universities can emulate each other's prowess in each and every field of intellectual activity. The Science Research Council has done its best to provide a framework within which, in scientific research, work in particular fields can be concentrated at the places at which it is done well. The advantages of such a policy are easily discovered. People are able to work in groups which are large enough to make important contributions to important and difficult fields. Students are brought up in environments in which there is at least a chance that scientists with established reputations will be heard and even seen to disagree. The experience of American science in its heyday (in the 1960s) goes to show that the free movement of teachers and students between a diversity of centres for creative intellectual work can be a powerful catalyst of innovation. It remains to be seen whether the Science Research Council in Britain can achieve anything like the pattern of rationality that it seeks, and there is at present very little sign that such an agency will emerge with powers to operate on a European basis, yet the fruits of such a development would be almost a sufficient justification in themselves of all the efforts spent in the past two decades on the construction of the EEC.

What this implies is that making a closely integrated intellectual community in Europe, necessarily more distant, is as important an objective as any other subsumed within the economic discussion of the past few years. From this point of view, however, science may have as much to contribute as it stands to gain. To be sure, the old fashioned notion current in the thirties that scientific understanding can without further preparation function as a kind of international adhesive is now quite discredited—the international competition for Nobel prizes is for that matter as conspicuous and potentially as destructive as the competition for medals in the Olympic Games. Yet there is a sense in which the mutual self-interest of people working on common problems drives them into methods of collaboration which should be able to serve as models not merely for the machinery of an enlarged EEC but also for some of the less tangible objectives which need to be defined. The way in which the past few years have seen the emergence of several learned societies which are organized on a thoroughly international basis—and one transcending EEC—is a particular but powerful example of what can be done.

It is natural to ask whether the scientific community should not deliberately set out to show how forms of international collaboration on a European basis could speed the arrival of a much more closely integrated European intellectual life. Is there not, for example, a case for extending in scale and to other disciplines the system by means of which the national academies of several European nations have in the past few years managed to move people from one country to another for short spells? Should not universities take it on themselves to collaborate with each other for efficiency in such obvious and important fields as the organization of

conferences, the publication of data and even the design of research programmes? These, after all, are jobs that could be done without the formal agreement of government or committee-laden societies. In short, there seems now to be a chance that the intellectual community as a whole, but scientists in particular, could help enormously to make the EEC more than just a customs union.

How will the potential of the European community for scientific research and technical development be most fruitfully exploited? In the immediate future, there is no doubt much to be done by the fuller development of some of the schemes for technical collaboration which have been worked out in the past few years within the EEC. In several comparatively humdrum fields, such as the provision of housekeeping services for science and technology, there is indeed an urgent need that the EEC should pursue more energetically than it has done a variety of sensible goals. It is perhaps easy to understand that the development of a European currency will be a slow process, but is there any reason why it should be such an apparently slow process for the national standards laboratories of the member countries, for example, to learn to take in each other's washing? This is one field, at least, in which it could be expected that the present work of six laboratories, or ten perhaps, could either be concentrated in one or two or, more realistically, could be shared out much more efficiently than at present among standards laboratories specializing in the kind of work which they do best. And is it not right to ask that one of the savings of manpower as well as money to be expected within the framework of the EEC should be a single standards organization, earning a good part of its living from fees levied on industries but otherwise commonly financed—and costing no more than, say, a quarter or a third of what the members and putative members of the EEC at present spend on work of this kind?

It is easy enough to compile a catalogue of circumstances in which the building of such common institutions would be at once beneficial and comparatively simple. Pollution research is an obvious candidate, for although the laboratories which are at present working in this field have local problems on their agendas, this is eminently a field in which the development of methods of measurement, methods of analysis and a general understanding of what issues are to be seriously considered are the most perplexing problems. And the dispersion of smoke from a tall chimney is unlikely to be determined by different laws in different countries. This, then, is another field in which it would make sense to aim at commonly financed research programmes, no doubt with some contribution from local funds. But there is a similar case for common participation and financing of programmes for research in food safety, standards for the safety of medicines, vaccines and other bio-medical products as they are called, the breeding of plants and animals, the provision of facilities for testing the performance of ships or of great hydraulic works, the observation, analysis and prediction of the weather and the exploitation of the continental shelf including the North Sea. There is no shortage of topics on which European well-wishers can hope to see the emergence of common institutions, promising at once a saving of resources and a better performance.

So why are some hats thrown only hesitantly in the air at this exciting prospect? Experience in the past twenty years in the development of common European

technical institutions has been disappointing and sometimes even embittering. The collapse of the Euratom organization has cast a long shadow, and it is only natural that meteorologists with national institutions which at present function smoothly and sometimes even efficiently should be chary of the suggestion that they should now work within the framework of a European organization. Why take the risk and, in any case, is not the World Meteorological Organization a sufficient assurance that there will be some measure of international collaboration when this is necessary? In the same way, the continuing difficulties within ELDO and ESRO are frequently taken as signs that there is only danger to be found in formal schemes for technical collaboration on a European scale. Fortunately, however, these admonitory examples are misleading. In retrospect, it is plain that Euratom, ELDO and ESRO have been disappointments largely because too much was expected of them. Euratom was endowed with all the best intentions, but it was never realistic to suppose that the member nations would continue to support both the common institution and their own national organizations for the development of nuclear power without letting chauvinism and the hope—sometimes illusory—of commercial advantage take charge of events. As a result, Euratom could hardly have avoided becoming a stalking horse for the national atomic energy agencies, each of which would have been offended if Euratom had been technically successful, but each of which assumed that it was free to complain at the inefficiency of Euratom. It remains an urgent need to find some civilized method for the development of fast reactors, but nobody would recreate Euratom for that purpose. Although some of the same jealousies are responsible for some of the difficulties with ELDO and ESRO, the most serious problem there is the artificial character of the prospectus on which each of them was assembled in the early sixties.

The morals to be drawn from these unhandy recipes are, in the circumstances, plain enough. First, no purpose is served by the development of common institutions for tasks which are unnecessary. Second, common institutions are most valuable when they can be seen, especially by those who contribute to the cost, to be credible substitutes for national institutions already in existence. In other words, the case for the common technical institutions that are most likely to be viable should be arguable on economic as well as technical grounds. Third, it is important that the development of common institutions should not be regarded as a means of safeguarding the interests of individual members of the EEC in maintaining, perhaps uneconomically, a stake in some technical field that should, by rights, be cheerfully abandoned. After all, one of the forces that brought Euratom to its knees was the conflict of interest between those concerned with the commercial development of nuclear power on a national basis and those who regarded Euratom as a means of entering this important field without too great a financial investment. In reality, however, the EEC is a potentially powerful rationalizing principle in advanced technology chiefly because it offers a means by which individual nations can concentrate on the fields in which they happen to be good without fearing that they are losing some vital advantage elsewhere. In this spirit, there are good reasons to distrust some of the schemes invented in the past few years for European collaboration in such things as the development of advanced computers or

machinery for telecommunications. On the face of things, it is hard to see why commercial enterprises by themselves should not be driven by purely commercial considerations to form consortia for these tasks, without prodding from the EEC as such. On the whole, the best subjects for formal common institutions are those in which the public interest is predominant.

So should the management of these common technical institutions be carried on from Brussels, along with the policy on the common currency and the agricultural policy? There is a case for seeking other ways, if only so as to anticipate what Europe will be like when the present bureaucracy has withered away, to be replaced by something much more like the politically oriented governments which are now familiar in the West. Moreover, the laboratories already concerned with work of the kind best suited to common programmes are already well known to each other and mutually respected. Between themselves, they could work out efficient means first of collaboration and then of integration. To be sure, it would be necessary that national contributions to the common budgets of these technical institutions should be recruited from governments with the connivance at least of the EEC, but there is no reason why this part of the job should not be comparatively formal. Certainly there is no need to fear that the development of each new common institution will entail the construction of yet another smart new glass and concrete office building in Brussels. Indeed, the technical community is by now so well accustomed to running its own affairs that it could well provide a valuable demonstration of how simply common institutions can be managed.

In fields where public money is at present used to foster the technical development of commercial industry as distinct from the public services, more teasing problems will arise. The difficulties are well illustrated by laboratories such as that at Harwell, where the British Government has invested substantial sums of money for the development of what was presumably intended to be a British nuclear power industry, and where it is now hoped that the same resources can be applied to the interests of a much more diverse group of industries. So far, the policy of diversification at Harwell and at similar public laboratories in Britain has been held to exclude contract work for foreign companies, however profitable that might be. The interesting question soon to be decided is when and how this policy of exclusive self-help should be eroded at least in favour of other European companies. The evolution of a sensible policy on these questions will necessarily be slow, but in the long run this is another important sense in which it may be possible to make fuller use of the resources which exist.

For the rest, there must be serious doubts about the pace at which purely technical collaboration will be possible within the EEC. After all, the organization exists because it has an underlying economic logic, but in due course the pattern of industry and technology will develop in such a way that formal collaboration between member nations in industrial technology will become unnecessary. Instead, member nations will buy each other's products and frequently will be unabashed that they do not manufacture them themselves. Expressed in the now familiar jargon, what this implies is that interdependence and not collaboration should be the goal.

The need to work out forms of collaboration in industrial technology is by far the most urgent objective of a policy on technology within the EEC, but in the long run it will of course be comparatively unimportant. Ten or twenty years from now, after all, the pattern of technology in Europe will be quite different. So too will be the pattern of industrial enterprises. At that stage the important questions will be to know how best to foster the development of scientific research of a more basic character. At that stage, the universities are certain to be intimately concerned with the development of a European policy. The question will again arise of the extent to which national institutions of higher education should be used for supranational purposes. Should British uni-

versities take in potential students from abroad, and on what terms? Should British or French grant-giving authorities provide money for research for deserving applicants from elsewhere? Should universities and academic research laboratories be staffed not simply by foreign nationalists but by the best people for the job, wherever they may come from? It is too soon to know precisely how these questions will be answered, but there is no doubt that in the long run methods must be found for integrating the university systems of Europe. That, after all, is the only way in which the intellectual potentials of Europe, only narrowly to be understood as six or ten countries, can be fully realized. And that, in the long run, is the only safeguard for European science.

SWITZERLAND

Searching for a Science Policy

Many of the difficulties inherent in the formulation of Swiss national science policy arise from the well-guarded autonomy of the cantons. Applied research seems to be particularly neglected at a national level.

AN analysis of Swiss science policy is difficult because in fact no general and systematic science policy has so far been adopted. About a month ago a very interesting report on this problem was released by the Organization for Economic Cooperation and Development (OECD), together with a set of pertinent recommendations. This report and the recommendations display a remarkable understanding of the special factors which have to be taken into account.

Special Case

What makes Switzerland such a special case as far as science policy is concerned? First it must be realized that the political structure of the country gives a large amount of autonomy and independence to the twenty-two cantons, each of which is responsible for all matters not explicitly attributed to the federal government.

A second characteristic of the Swiss situation, which probably reflects the opinion of the general public as well as that of the "establishment", is a resistance, almost an allergy, to any form of state intervention. This attachment to the *laissez faire* theory makes it politically very difficult to rationalize and to orient efforts at a national level.

A third unique feature of Swiss science policy is the fact that although Switzerland, like many other industrial countries, spends about 2 per cent of its gross national product on research and development, only a quarter of that amount is spent by the government (federal and cantonal) and the remaining three-quarters is spent by private industry.

National Fund

There are several official agencies which are concerned with the government support of research and of higher education and most of these have been created in recent years.

The Swiss National Fund for scientific research came into being in 1952 as the result of the efforts of the scientific community. Its first president was Professor A. von Muralt and its function is to distribute the funds allocated by the federal government for the support of basic research in the universities. Although receiving all its finances from the government, it is legally a private foundation—a unique case for a national granting agency. The National Fund has, however, certain obligations to the federal government, in

particular that of supporting research according to the general guidelines of national science policy. Each year the proposal for the allocations of its budget to different scientific domains must be approved first by the Swiss Science Council and then by the federal government. Another interesting aspect of the work of the National Fund is that it encompasses medicine, the social sciences, and the humanities, as well as natural sciences.

The fund is now presided over by O. Reverdin, professor of Greek, member of parliament and also president of the assembly of the Council of Europe. The members of the National Fund are recruited from the outstanding scientists in the country and are themselves directors of principal university research centres. Despite the difficulties inherent in an objective evaluation of scientific grant applications in a country as small as Switzerland, the judgments of the National Fund are generally respected in the scientific community. The system is considered the best possible until the creation of a European review board which has often been suggested in the past.

The sum allocated by the federal government to the National Fund was 2 million Swiss francs in 1952, 23 million in 1964 and 70 million (or £7 million) in 1970. But Switzerland's contribution to international scientific institutions, such as CERN, is paid directly by the government and is not taken out of the budget of the National Fund. In line with the situation in some other countries, the amount of money requested has increased faster than the budget, and the National Fund has recently been unable to fund some projects which it considered scientifically worthwhile. In this new situation, the criteria of scientific merit are no longer sufficient for the evaluation of grant applications. A list of priorities is definitely needed at national level.

Choosing Priorities

The task of deciding on such priorities, and therefore of formulating guidelines for the orientation of research, belongs to the Swiss Science Council. This body was only set up in 1965 and is, according to a law enacted in 1968, "the consultative organ of the federal government for all questions of national science policy". Its task is to formulate a "national policy for science and research and to suggest measures for its implementation". Although this appears

to be a clear mandate for the general and systematic approach which has been lacking in Switzerland so far, there are several reasons why the Swiss Science Council has not yet found itself in a position to propose such a general policy. First, the importance of cantonal autonomy and the resistance to intervention have kept the council in a reactive rather than an active state. Second, the council has spent a large amount of its time during the past five years on questions concerning university policy. In particular, it has proposed a new system which has made possible federal support of the cantonal universities. The chief reason for the lack of a general science policy is that a large preparatory effort is necessary before any step can be taken in that direction. Such preparation has, however, now begun.

The Swiss Science Council is presided over by Professor K. Schmid of Eidgenössische Technische Hochschule (ETH), Zürich. It consists of twenty members chosen by the federal government not only from the scientific community but also from the fields of politics, economy and labour. The working body of this council is a committee of six members and the council is assisted by a secretariat which includes four scientific assistants with academic degrees. The important ministries of the government (or "federal departments") also have observers on the council.

Because all universities in Switzerland, with the exception of the two federal technical schools of Zürich and Lausanne, are under the authority of the cantons, there was a need for an agency to be in charge of the difficult task of coordination. The University Conference came into being in 1968 for that purpose. The cantons, the universities, the administration, the student unions and industry are all represented and the University Conference is a meeting point for the conflicting tendencies of cantonal autonomy and national planning of university policy.

Also in 1968, the Division for Science and Research was set up within the Ministry of the Interior of the federal government. This division is something of an executive partner for the various agencies mentioned above and in particular is responsible for carrying out the recommendations of the Swiss Science Council. It is, for instance, through the facilities of the Division for Science and Research that the federal subsidies are channelled to the cantonal universities.

A list of the agencies concerned with science policy in Switzerland must also include the Commission for the Encouragement of Research, which operates under the auspices of the Ministry of Economy. It has a budget of only 2.5 million Swiss francs per year, and sponsors applied research in the universities which is relevant to the economy as a whole. This meagre budget contrasts sharply with the much larger amounts of money allocated to this type of research in other countries and the greater importance generally attached to it. The whole question of government support to applied research is at present being reinvestigated in Switzerland.

Towards a Change

In view of the feelings of cantonal autonomy and resistance to state intervention, it is not surprising that the decisions concerning research policy and policy for higher education have been arrived at one by one, and chiefly by consensus (if not by compromise), rather than as the reflexion of a general national science policy with well defined objectives. And there are probably many in Switzerland who still think that this is just as well!

Recently, however, several circles in Switzerland have begun to ask for a new approach to the question. They have argued that although the pragmatic, case by case approach probably conforms more to Swiss traditions, there is now a need for a systematic and more global conception

of science policy. They feel that the expenses are becoming so large that the encouragement of research should become more selective and that national priorities should be decided upon. This attitude is quite similar to that expressed recently in Canada by a parliamentary commission whose report criticized the Canadian Council of Sciences for its lack of intervention and for leaving science policy too much to chance. The recommendations of the OECD experts to the Swiss government also advise much more central planning for both research and university policy. As Switzerland is only now beginning this task, she should benefit from the experiences and mistakes of other countries.

Tasks for the Future

Three main tasks will confront the Swiss Science Council during the next few years.

University policy. The eight cantonal universities of Switzerland, with a total student population of about 40,000, will have to be rapidly developed if the country wishes to maintain its unique policy of free entrance to the universities for all who have a "maturité" degree. The introduction of restriction or selection in certain faculties, such as medicine for example, might be necessary in the near future but would be contrary to the policy followed so far. University development will create financial difficulties for the cantons and will highlight the need for increasing federal support for the cantonal universities. The possibility of a complete federal financing of the "Swiss universities" (Hochschule Schweiz) might even be envisaged. Other aspects of the same problem include the choice between the development of the existing eight universities or the creation of new ones. It is also evident that the expansion of post-graduate teaching and research will have to be concentrated in specified fields at certain universities.

Research policy. A thorough study of the motivations for research will also have to take place. For a small country like Switzerland, only three principal motivations seem justified. First, research for the sake of it, in which case priorities will be very much dictated by the chances of success in a given field; second, research as a response to the needs of society, which include social medicine, protection of the quality of life and so on; and, third, research as a contribution to the development of the economy. The selection and justification of these priorities, the choice of criteria, and the study of the methodology involved in such a process will require a large amount of work. Consultation in several quarters, both scientific and non-scientific, about the need for the development of specific fields of research, was started last year and the results of this study will be evaluated later on this year.

Applied research. As mentioned earlier, the organization of support for this sector of research is being reinvestigated in Switzerland. In proposing new structures it will be necessary to take into account the great resistance on the part of Swiss industry (mostly the very large companies) to any state intervention even to support applied research. These structures will have to reflect the growing importance of applied research with non-industrial motivation, in particular in the field of protection of the environment. It is possible that, as in other countries, a new granting agency for applied or technical research—distinct from the National Fund—will have to be created. Such a solution has in fact recently been recommended.

One conclusion which can be drawn from this analysis is that Switzerland, confronted like many other European countries with the new and difficult task of formulating a science policy, could benefit from a better exchange of information with those responsible for such work in other countries. Perhaps informal workshops or symposia could be organized where those directly responsible could share their experiences, hopes and frustrations.

POLAND

Where Science Stands Now

Polish science has advanced on a wide front in the years since the end of the war but international collaboration is still an essential factor in some fields.

THE disastrous effects of the Second World War on Polish science have happily now been removed both because of the extensive support given by the Polish government and because of the efforts of the scientific community itself. Since 1945 the government has actively promoted the foundation and growth of both universities and research institutions and signs of a Polish scientific revival were evident within a very short time after the war ended.

Undoubtedly the turning point in Polish mathematics came a long time before the Second World War—in 1918—when Professor Janiszewski of Warsaw University outlined a programme of mathematical research whose object was to secure a prominent place for Poland in the world of mathematics. His programme covered a fairly narrow spectrum—chiefly mathematical logic, set theory and topology, and functional analysis—but by about 1939 most of his goals had been achieved and Polish mathematics had become well established. In 1945 it was necessary to start again and also to extend mathematical research into such fields as modern analysis, algebra, probability theory and applied mathematics. Nowadays Poland's most important seat of mathematical learning is the Institute of Mathematics at the Polish Academy of Sciences in Warsaw; the institute also has a number of branches in other cities. There is extensive cooperation with bodies concerned with the national economy and the institute has figured prominently in matters ranging from the development of a computer method for the solution of transport problems to the study of a mathematical model of leukaemia. A vital role is played by the Polish Mathematical Society, which has more than 1,000 members, and Polish mathematicians would be the first to admit that international cooperation has an important part to play. Links with the UK are particularly strong and Professor I. Sneddon (University of Glasgow) is now a foreign member of the Polish Academy of Sciences. Two Polish mathematicians, K. Kuratowski and W. Nowacki, have also recently been elected Fellows of the Royal Society of Edinburgh.

Research in physics has been carried out in recent years chiefly at institutes in Warsaw, Poznan, Silesia and Wroclaw, most of which are connected with the Polish Academy of Sciences. Some of the most important achievements, as far as solid state physics is concerned, have been in the field of semiconductor physics, particularly in the study of the surface properties of semiconductors which are so important for the successful operation of devices with very thin semiconducting layers. One of the features of Polish research in spectroscopy of all types has been the construction of a high pressure reaction vessel to facilitate the study of electron paramagnetic resonance in samples at high pressures. The results of low temperature research are also promising. Much of this has been carried out at the Institute of Low Temperatures and Structural Research in Wroclaw, where development work in cryotechnology includes the improvement of both insulation and cryogenic instrumentation. Research on superconductivity, particularly in metal mixtures, is well established, and superconductor thermometers for the measurement of temperatures near that of liquid helium have been actively developed.

An example of cooperation in physics between socialist countries is the International Laboratory of High Magnetic

Fields and Low Temperatures set up in Wroclaw by an agreement between the academies of sciences of Poland, the USSR, Bulgaria and the German Democratic Republic. The chief interests of the laboratory are the phenomenon of superconductivity in general and the production of large magnetic fields with superconducting magnets.

Nuclear physics is also an important research topic and the chief centres were founded by A. Soltan in Warsaw and H. Niewodniczanski in Cracow. Research in high energy and elementary particle physics has also evolved from cosmic ray work undertaken at both these centres, and a considerable amount of cosmic ray physics is still carried out at Lodz. Among the experimental facilities available to the nuclear physicist are a 120 cm cyclotron in Cracow and an 8 MW nuclear reactor in Warsaw.

Much of the experimental high energy physics is carried out at the large accelerator laboratories (Dubna and Serpukhov in the USSR and CERN at Geneva) where Polish physicists are active in international teams. Subjects of special interest in Poland include hypernuclear physics and strong interactions. Several Polish theoretical physicists are working on high energy problems—for example, J. Werle, who is well known for his work on phenomenological theories of strong interactions.

In the years before the war, astronomy in Poland was chiefly devoted to such problems as the classical theory of orbits but, later, research of a more astrophysical nature became a dominant feature. Polish astronomers gained a sound international reputation for their work on stellar evolution (especially of binary stars) and on observational photometry and spectroscopy. Most of the important work has been done at the astronomical institutions in Warsaw and Torun which are supported both by the Polish Academy of Sciences and the two universities.

Research in the chemical sciences has developed rapidly during the past twenty years in a large number of research centres and institutions. In particular W. and B. Trzebiatowski lead a large group at Wroclaw with interests in the chemistry of rare metals and the structure of such inorganic compounds as transition metal hydrides and co-ordination compounds. Low temperature research was started at Wroclaw only a few years ago and several new phenomena, such as the ferromagnetism and antiferromagnetism of certain uranium compounds, were quickly discovered. As far as physical chemistry is concerned, many workers are continuing the work of the late Professor W. Swietoslawski at the Institute of Physical Chemistry of the Polish Academy of Sciences where the chief subjects of interest are thermochemistry, azeotropy, ebulliometry and cryometry. B. Kamienski was responsible for initiating the investigations of the physical chemistry of surface phenomena at Cracow and work on the same topic has also been started by other groups at Warsaw and Lublin. Several fields of research are of particular technological importance—for example, research into methods of purifying terephthalic acid and investigation of corrosion. Corrosion is, of course, an expensive worldwide problem and, as well as developing methods for the direct measurement of the speed of corrosion, Polish physical chemists have also studied the corrosion inhibiting effect of amines, alcohols and primary aliphatic acids on steel. Problems of industrial waste have been solved to a

certain extent in Poland and during the past few years it has become possible to make use of slag heaps in industrial regions for agriculture and forestry; this has been a considerable economic achievement in view of the large quantities of this type of waste throughout Poland.

The efforts of a large number of organic chemists have led to the construction and successful operation of two industrial plants producing catalysts. Work on the poly-recombination of paraffin hydrocarbons has produced a product which can be used either simply as a wax or as an electrical insulating material. Polish researchers are also active in most of the Earth sciences. Research in geography is carried on chiefly at the Institute of Geography in Warsaw; a hydrographic map of Poland has been completed recently and a far-reaching study of agriculture throughout the country commenced. There is also considerable interest in the geography of Polish industry and some of the geographic work is aimed at understanding the division of Poland into climatic regions on the basis of the heat-balance structure.

The geological activities of the Institute of Geological

Sciences are chiefly concerned with the formation and occurrence of useful mineral deposits—such knowledge is obviously of great economic importance. The institute has also been concerned with the documentation of surface processes such as landslip, erosion and accumulation; extensive use has been made of photogrammetric methods of surveying in this context. Research in geophysics and geodesy is principally the responsibility of the Institute of Geophysics and many of the seismic measurements which have been made there have yielded useful information about plate tectonics and the structure of the Earth's crust.

There is a temptation to think that the centres run under the auspices of the Polish Academy of Sciences are the only places in which research in Poland is undertaken. In fact two other sectors are also active, namely the Institutions of Higher Education and the Industrial Institutes. All three cooperate closely with each other and quite frequently they carry out joint investigations. The Academy can best be regarded as providing the overall guidelines for Polish science as a whole.

ESRO and ELDO

Point of No Return

The future of ESRO and, particularly, of ELDO hinges on any agreement which may be made to join NASA's post-Apollo programme. Europe almost certainly cannot afford both participation in the shuttle programme and the continuation of ELDO, but it has been suggested that a firm undertaking by NASA to launch all ESRO satellites (including applications satellites) would pave the way to complete transatlantic cooperation,

It is beginning to look as if the joint European space effort represented by ESRO and ELDO has reached the stage where its momentum will continue to carry at least part of it forward despite the parochial attitudes of some politicians. Many difficulties lie ahead, but the successes of recent years (particularly in ESRO) have clearly shown that collaboration is the best path for the European countries to follow.

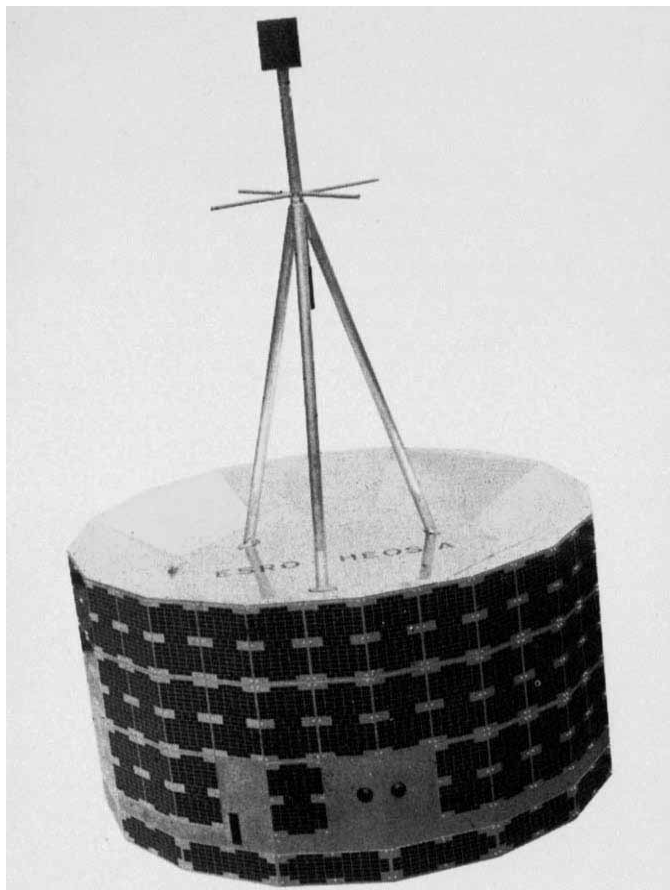
The outstanding success of ESRO, compared with the rather more limited achievements of ELDO, offers an interesting comparison of two ways of organizing large international projects, from which many morals can be drawn. ESRO is a very efficient organization, and much of this efficiency stems from the strong central management whose powers derive from the ESRO convention. Every three years, the ESRO budget is approved by the unanimous vote of all members, and the voting is always on a one country/one vote system—thus Denmark (contributing 2.23 per cent of the 1970 budget) has as much say as West Germany, which contributes ten times as much to the ESRO funds. Such a vote is to be taken this year, but once the financing has been agreed the professional management of ESRO will be free to allocate funds to projects as necessary, with only a two-thirds majority vote of member countries required even for major policy decisions. Contracts are allocated by ESRO, and it is usual for ESRO project management teams to assist in the smooth carrying out of the contracts. Finally, it is the responsibility of the central organization to test the satellites, and to provide telemetry, computers and so on with which to obtain and analyse data from them.

Three Out of Four

Four ESRO satellites have now been launched. Three have been overwhelmingly successful, and two are still

in operation today. These are ESRO-II, launched on May 17, 1968, ESRO-I, launched on October 3, 1968 (re-entered in 1970), and HEOS-I, launched on December 5, 1968 (see Figure). The rather more disappointing ESRO-Ib was launched on October 1, 1969, and has since re-entered the atmosphere. But even this satellite was by no means a failure, returning valuable data on ionospheric and auroral phenomena, and suffering only by comparison with its three sisters. The early impetus provided by the rapid development of these satellites provided the ideal boost for ESRO as soon as it was formed. The three major projects now under way should provide a similar impetus for the next generation.

The advanced projects are for HEOS-A2, a satellite to study the magnetosphere due for launch in December 1971, TD-1, a satellite of great interest to astronomers, as well as being the most complex ever planned outside the USA and USSR, which should carry X- and gamma ray telescopes into orbit in the spring of 1972, and ESRO-IV, another satellite to study the upper atmosphere, planned to become operational in the autumn of 1972. All these satellites have been, or will be, launched by US rockets provided by NASA. Minor problems, often caused by the need to distribute contracts with some political motivation so that each member country receives its fair share of work, have been smoothed over, and although the use of 50 per cent of ESRO funds for "internal purposes" is a figure often quoted by detractors of the European space programme, this figure includes guidance, telemetry, one of the largest computer complexes in Europe, and projects as "external" as a plasma research institute and sounding rocket experiments from ESRANGE at Kiruna in Sweden. For the future, the logical development is a diversification of ESRO work to include applications satellites, notably communica-



The ESRO HEOS-1 satellite.

tions satellites. But this development is inextricably linked with politics and the question of whether or not to develop a European launch capability.

Disappointing Contrast

Unlike ESRO, ELDO has suffered not only severe cost escalation but also expensive failures. It is oriented towards only one project whereas ESRO has many projects in hand. There is no technically powerful central organization possible under the ELDO convention, simply a secretariat numbering about 300. This is a direct consequence of the military implications that used to be inherent in the liquid fuelled rockets from which the ELDO launcher is derived. Now, of course, it is clear that these rockets are militarily useless, but each country still hoards its rocket know-how. As soon as one participant finds that its share in the project requires more money than originally planned, all the other participants cry out for more on principle—the lack of firm central control is only too evident. The system has been described by frustrated believers in the European space programme as a perfect example of cost escalation by positive feedback.

Worse even than the limitations of the day to day running of ELDO is its lack of a clear goal. Originally, all that was planned was a launch capability of 2 tons into low orbit—but as far as ELDO's engineers knew, the 2 tons might be just concrete. Having stretched the project to a project and a half by the proposal to add fourth and fifth stages to the launcher to inject 200 kg satellites into geostationary orbit, no full study was made of the nature of this payload either. It is far too little for an economically sensible communications satellite, and what else requires such an orbit?

Nevertheless, Germany, France and Belgium retain much

enthusiasm for the ELDO launcher, though France is naturally pleased with its much smaller Diamant, so far without a failure to its name. Britain, having opted out of ELDO, continues, to the annoyance of many on the continent, with a half hearted Black Arrow project which, if it ever does work, will only duplicate Diamant's capability.

The launcher question is, however, seen in a different light when applications satellites are considered. So far, NASA has provided help at all stages of the development of ESRO satellites, as well as providing the launchers. But there is a serious doubt about continued US aid with, say, communications satellites which will be in direct competition with satellites of the US led global Intelsat consortium. At this stage the question of the space shuttle raises its head. NASA is keen to find partners in the shuttle programme, but Europe cannot, it seems, afford both to participate in the shuttle programme and continue with ELDO. So the offer may be made to join in the shuttle programme provided NASA promises to launch all ESRO satellites, including communications and other applications satellites.

Understandably, the US is not convinced by this argument. Today, Britain and France are spending money on their national and collaborative space programme at the rate of 1.0 and 2.5 dollars per person per year. America still spends 15 dollars per person per year on space, even though this is half the peak expenditure in NASA's heyday. It is even more annoying for the Americans and our European partners when Britain declares that there is no money for Britain to participate in a European portion of the shuttle programme, but promptly backs BAC's offer of £150,000 with a further £150,000.

Continuing Success?

Even with willing participation on all sides, it is difficult to see how a useful contribution to the shuttle development could be made by Europe, simply on logistical grounds. Anything interesting enough to merit large expenditure would be very difficult to prepare more than 3,000 miles from the rest of the shuttle, if the whole vehicle is to work properly when the parts are put together.

So what of the future? ESRO's continued success seems assured, provided only that there is united (including British) support for the work on applications satellites. Collaboration by individual countries with NASA also seems to have a rosy future, as shown by the success of the UK series of satellites, and the recent launch of Explorer 42 by an Italian team. Logically, ELDO and Black Arrow should both be scrapped, and the savings used to participate in the shuttle or some other NASA project, with the rider that in exchange NASA must provide ESRO with launch facilities. These would include ideas already in the pipeline for a European communications satellite, an air traffic control satellite, and a meteorological satellite. It will certainly be interesting to see the reaction of the member countries to the official budget requirements for these satellites, which they must pass this year. The logistical problems remain, but should not be insuperable for a technology which can send men to the Moon. Politically, Britain may well offer or be asked to join in Europe's share of the shuttle as part of the complicated give and take of the Common Market entry negotiations.

ESRO came in with a flourish, producing three superb satellites in a very short time. Following a period of retrenchment and some internal bickering, the way now seems visible, if not yet clear, for the next wave of production. But all must remain largely speculation until the meeting of the ESRO council in midsummer to consider the next three year budget, and the funding of the applications satellite programme.

FRANCE

At the Crossroads

One of the consequences of France's Sixth Plan is that research in social science and the life sciences will grow at the expense of, for example, nuclear physics. In general, the emphasis will be on improving the quality rather than the quantity of research.

FRENCH scientific research has been passing through a period of uncertainty which started in May 1968 and is still continuing. It is now possible, however, to discern trends in the future orientation of French research by examining the research priorities selected for inclusion in the country's Sixth Plan (1971 to 1975), which should be approved by Parliament in June and made effective immediately.

Feelings about the launching of the new Plan are far from enthusiastic (*Nature*, 230, 420; 1971), although it would be unfair to say that the last five years have been unproductive as far as research is concerned. The fact is that research is no longer considered a priority feature of the economy. Although the Fifth Plan (1965 to 1970) provided for fairly extensive research and development spending—by 1970 the rate of investment was to have risen to 2.5 per cent of the gross national product—the rate finally adopted was 1.84 per cent of the GNP, clearly short of the target and in fact lower than the comparable figure for 1965.

Development Favoured

The Sixth Plan now provides for overall investment in research and development corresponding to 2.45 per cent of the gross domestic product (the GDP is a unit of measure used by French and other economists, and corresponds to a figure 10 per cent less than the GNP). This, then, is the goal for 1975. The proportion of the total research and development funding allocated to development should rise steadily from the present level of 48 per cent to about 52 per cent. The total sum earmarked for capital investment appropriations, including most of the non-military research and development appropriations but excluding civil aeronautical projects such as Concorde and the Airbus and the activities of the Centre National d'Etudes des Télécommunications, will be 21,400 million francs for the quinquennial span. This amounts to about £320 million per year, an annual increase of 9.5 per cent over the 1970 research and development budget. Even this figure, which was settled after some government arbitrage in April, is lower than the one originally proposed by the research committee of the permanent Commissariat au Plan.

The chief research guidelines set out in the Plan can be fairly easily summarized.

- Research and development are to have a regular role in the planning of industrial research.
- Money is to be allotted to research to improve the general quality of life.
- Spending on basic research will proceed at the same average rate as that for the whole research and development budget.

It follows that particular fields of research will clearly be favoured—for example, the human sciences as well as the life sciences. Indeed, these disciplines are to be expanded at a much higher than average rate, whereas some research, such as nuclear physics, will definitely be slowed down. And unlike the Fifth Plan, the Sixth will not provide for the building of many new laboratories. The emphasis will be,

instead, on improving the quality of research at the expense of material expansion.

Technological Innovation

As far as research is concerned, the Sixth Plan is the outcome of the efforts of the Délégation Générale à la Recherche Scientifique et Technique (DGRST) who, in drafting the original research programme, attempted to reconcile and mediate the demands of state research organizations (especially those concerned with basic research), the Ministry of Industrial and Scientific Development and, finally, industry itself. To be sure, the accent is on technological innovation and large sums of public money are earmarked for industrial research through the procedure known as *l'aide au développement*. This allows the government to lend as much as 2,000 million francs to industry to finance promising innovations. If the innovations subsequently prove to be profitable, the borrower repays the loan to the state; if not, the state simply loses the gamble.

There are, however, two interesting changes. First, research programmes such as aeronautics, space and atomic energy, which have been heavily supported in the past, will evidently no longer be favoured because they are not regarded as vital to the implementation of industrial policy; no important new projects are planned in these areas of research. The emphasis has shifted to other subjects, such as chemistry and mechanical engineering, which are essential to the nation's economic growth and in which there is much technological problem solving to be done.

Second, industry itself is called upon to increase considerably its own contribution to the financing of research and development from 32 per cent to 35 per cent by 1975. This is necessarily something of a gamble (or else an act of faith) because there is no guarantee that such a target can be reached until French industry has emerged from its period of reorganization which was a consequence of its previous overspecialization.

Tax Exemptions

It is likely, too, that there will be a considerable amount of lobbying during the next Plan in favour of significant tax exemptions for those companies investing in research and development. Yet it is precisely on policy matters such as this that the Plan has the least to say. But it must be said that although the Plan is not overwhelmingly ambitious in terms of research, it does have some very good features, particularly the choice of the institutions implementing the Plan. Since 1969, for example, the Ministry of Industrial and Scientific Development has been in charge of most of France's non-military research organizations. Its dependent, DGRST, is responsible to a certain extent for co-ordinating scientific policy at the interministerial level, but the ministry itself has yet to work out its *modus operandi*. The recent setting up within the Ministry of a department to deal with liaison between the various research pro-

grammes (see *Nature*, **230**, 346; 1971) has actually upset the executive structure for implementing science policy, chiefly because the new unit's terms of reference are notionally the same as those of the DGRST.

Formula for Change

Several of the research organizations themselves are also destined for change. The Centre National de la Recherche Scientifique (CNRS), for example, is to introduce a new method of financing fundamental research this year. By means of *actions thématiques programmées*, interdisciplinary research on specific projects—already favoured by the Plan—will be supported. This seems to mean that the CNRS is reacting to certain criticisms and is now committing itself to some measure of selectivity; but it remains to be seen if the CNRS can work out a scientific policy by means of which it could define its objectives more clearly. The CNRS is having to bear more and more of the brunt of financing university research, because the universities are facing ever increasing money problems.

The Commissariat à l'Énergie Atomique (CEA) is slowly recovering from the serious financial reverses which it suffered during 1969 and 1970. The CEA's biggest problem at the moment is its relationship with industry; one possible solution is to establish an engineering corporation which would have the vital task of facilitating the introduction to industry of techniques developed and owned by the Commissariat.

CERN

New Lease of Life

The decision to build a large proton accelerator in Europe has been taken after several years of discussions during which the project seemed to be in jeopardy on more than one occasion. Some radical rethinking of national nuclear physics programmes will undoubtedly follow.

THE bustle of activity at the European Organization for Nuclear Research (CERN) during the past year has helped to ensure that the organization will continue for several further decades to be a fine example of a viable international research body. Even the protracted discussions on the 300 GeV accelerator, which at one time seemed certain to split the CERN membership, appear to have turned out for the best in the long run. The accelerator design finally adopted is undoubtedly the best from both the scientific standpoint—it is very adaptable—and from the financial point of view because it probably represents the optimum value for money.

The decision earlier this year to go ahead with the building of a large accelerator at the present CERN site at Meyrin, Geneva, was the culmination of more than seven years of patient work and planning which looked several times as if it would never come to fruition. It was during 1963 that the European Committee for Future Accelerators first urged the construction in Europe of a high intensity 300 GeV proton synchrotron. The first design study was published in 1964 and the cost was estimated (at 1969 prices) to be about 1,900 million Swiss francs (~£190 million). A state of stalemate then existed until 1968 when the UK government announced that it would definitely not participate. In an attempt to keep the cost of the project to the participating countries at a reasonable level, the budget was then revised to 1,430 million Swiss francs by pruning some of the money earmarked for early experimental facilities.

The appointment of Dr J. B. Adams as director of the 300 GeV project in 1969 led to a complete rethinking of

What Future for Space?

As far as the Centre National d'Études Spatiales is concerned, there is still considerable doubt hanging over the fate of various space projects, such as the Franco-German "Symphonie" communications satellite (*Nature*, **230**, 422; 1971) which has been seriously delayed. A D2A satellite was successfully launched from the Guyana base in April, but the huge cost of supporting the South American launch facility is not justified by the present level of activity there.

Faced with the possible options, French scientific policy in 1971 is evidently unsure of the right way to go. During the years between 1954 and 1968, which was the period of large scale development of research potential, the accent was on investment in trained personnel—the celebrated Landucci report, published in 1956, was entitled *To Prepare for the Future, Invest in People*—but now that this investment has been made, albeit somewhat unevenly, the emphasis has shifted to the ambiguous and even controversial notion of research worker "mobility". It is true to say that France's scientific institutions tend to lead an isolated existence cut off from the rest of the economy, but they are also in danger of a slow death if they cannot recruit young people and if a more effective scheme for the utilization of research personnel is not developed for the country as a whole. There are now 80,000 research workers in France, and the Plan aims to create 15,000 new jobs in the public sector and 10,000 in the private sector. At least, that is the goal for five years from now.

the whole design and the "missing magnet" concept was born. An accelerator built to this design would be capable not only of delivering a peak energy of 1,300 GeV if all the available magnet spaces in the ring were filled with superconducting magnets, but also of producing more modest maximum energies if financial or scientific considerations so demanded. But still there was the prospect of the CERN organization being split in two, with only six members willing to support the new project. A new problem also reared its head—that of deciding which of the list of five possible sites offered by the member states was to be used. Finally, an idea was mooted which not only removed the delicate problem of site selection but also reduced the cost over the eight year construction period to not more than 1,150 million Swiss francs (Table 1). One particularly attractive feature of the new proposals was that some of the total cost could be reduced by building the new accelerator close to the site of the present 28 GeV machine which would then serve as the injector. The approximate budget for the eight year period is shown in Table 1 in which the new money over and above the amount the member states would probably have contributed anyway is shown. Eventually ten of the twelve CERN member states agreed to participate in the project, the exceptions being Greece and Denmark.

New and Old

Digging on the new site (Fig. 1) which straddles the border between France and Switzerland is expected to begin

Table 1 The Estimated Budget for CERN during the Next Eight Years

	Existing facilities (millions of Swiss francs)	New accelerator (millions of Swiss francs)	Total (millions of Swiss francs)	New money (millions of Swiss francs)
1971	330	30	360	20
1972	320	105	425	85
1973	310	165	475	135
1974	310	180	490	150
1975	310	175	485	145
1976	310	165	475	135
1977	310	165	475	135
1978	310	165	475	135
Total	2,510	1,150	3,660	940

New money calculated from an agreed plateau budget of 340 million Swiss francs which would probably have been spent at CERN in the absence of the new accelerator. The total new money required is about £94 million.

in the late summer and detailed geological soundings on the site have already been started. Although the size and internal dimensions of the tunnel (diameter 2.2 km) are already frozen, there is still a certain amount of flexibility in the design of the accelerator itself although this must be finalized by the end of the year so that the principal contracts can be placed with manufacturers throughout Europe.

Not surprisingly the new accelerator, or super proton synchrotron (SPS) as it has now been dubbed, has dominated the CERN scene during the past months but this should not be allowed to overshadow the other work being carried out, which is no less vital than it ever was and is unlikely to be snuffed out by the emergence of the new accelerator. The recent completion of the intersecting

storage rings (ISR) and the achievement of storing a 4 A current of circulating protons in a clean and tidy fashion for several days have in many ways exceeded the wildest dreams of the designers and builders, who certainly did not expect successful operation quite so quickly. As it has turned out, it has proved difficult to keep physicists away from the experimental area although the experimental programme does not start officially until later in the summer. The first lecture on experimental results obtained with the storage rings was, in fact, given at CERN earlier this month.

It is also probably true to say that both the CERN 28 GeV proton synchrotron (which is to be the injector of 10 GeV protons into the new machine) and the 600 MeV synchro-cyclotron—the principal experimental facility at CERN during the years in the 1950s before the 28 GeV machine was completed—will continue to provide first class experimental results for many years to come. In particular, the relatively low pulsing rate (1 per 5 s) of the super proton synchrotron will mean that about 60 per cent of time on the existing CERN synchrotron will be available for 28 GeV proton physics. The intersecting storage rings will, of course, have to be filled daily, but this operation is not expected to take more than 30 minutes each morning. There is, however, likely to be a shift of emphasis in the 28 GeV physics which will be carried out in the future.

With the considerable reduction in the pressure to catalogue the properties and interactions of the elementary particles and to conduct experiments which have, in concept at least, a predictable outcome, the opportunity will come to carry out rather more esoteric experiments. It has sometimes been difficult to justify the expenditure of machine time on this type of work in the past, not because the experiments were not interesting and valuable but because their



Photo: CERN]

Fig. 1 The site proposed for the new CERN proton synchrotron at Meyrin, Geneva. + + +, Swiss-French border; — — —, the old and new accelerators.

ultimate result was too uncertain and the experimental resources were too limited. A good example would be a concerted search for the Dirac magnetic monopole—a very highly ionizing particle if it exists, but one for which there is no experimental evidence at all at present. It might also be possible to allocate quite a high proportion of the 28 GeV machine time to experiments specifically designed to look for heavy leptons for which there is already some tentative evidence^{1,2}.

Even the 600 MeV synchro-cyclotron, obsolescent by modern standards, is being modified so that the beam current of the protons which it accelerates can be increased by a factor of ten. The prospect seems to be that the machine will be of significant use for at least the next 10 to 15 years for experiments involving nuclear structure determinations and the study of muonic and pionic atoms and isotopes. Many more low energy scattering experiments with protons, pions and light nuclei, for example, are also likely.

Necessary Extras

An important feature of the work at CERN is the design or construction of the peripheral equipment without which successful accelerator experiments are not possible. A measure of the use which is likely to be made in the future of the 28 GeV accelerator as a machine in its own right is the number of pieces of experimental equipment which are nearing completion at CERN or about to be used in an experimental programme.

The new 4 m heavy liquid bubble chamber 'Gargamelle', for example, was handed over to CERN by its builders earlier this month. Gargamelle was designed and manufactured in France and its completion marks the fruition of six years of work initiated by Professor A. Lagarrigue, director of the linear accelerator laboratory of the Ecole Normale Supérieure of Orsay. Three laboratories—Saclay, Orsay and the Ecole Polytechnique of Paris—were responsible for the development work which took place under the aegis of the Commissariat à l'Energie Atomique. Among the first experiments to be carried out with the new chamber will be a neutrino experiment and a low energy pion scattering experiment.

By the end of 1972 there is also the prospect of the completion of both the 3.7 m superconducting European bubble chamber and the Omega Project, a versatile spark chamber array with which it should be possible for several experimental groups to do different experiments without extensive reassembly and rebuilding of apparatus.

The expenditure at CERN during the next few years (Table 1) assumes a considerable saving in the day to day expenses of the laboratory. The booster for the 28 GeV proton synchrotron is now a definite commitment which will increase the intensity of the beam to more than 10^{13} protons per pulse by the end of 1971 or early 1972. The expenses entailed in running it will, however, be under constant scrutiny and efforts by experimentalists to make use of it as an additional 800 MeV facility in its own right, thus incurring more expenses, are likely to be strongly resisted. But what are the other ways in which considerable financial savings can be made? Certainly a rationalization bubble chamber and spark chamber scanning facilities are very likely. It is widely felt that many more of these scanning activities could profitably be undertaken in national laboratories and universities and there is also some pressure to make sure that more of the development work associated with scanning and scanning equipment is done at a national level.

National Spending

During the rather drawn out discussions which took place at a national level before the final decision about the

new accelerator was taken, it became clear that cutbacks in the spending on national high energy physics programmes were inevitable. This was particularly so in the UK where it soon became clear that one of the consequences of joining the new project would be a fairly severe but unspecified pruning of the Daresbury and Rutherford accelerator laboratories and the whole nuclear physics programme. Although the Nuclear Physics Board of the Science Research Council is not expected to decide until the end of this year what the policy for the next five years or so will be, some trends are becoming clear. At the Rutherford laboratory, home of the 7 GeV proton synchrotron 'Nimrod', a swingeing investigation seems to be in progress to find ways in which the annual budget of about £7 million can be reduced. Costs involved in administration, engineering, travel, upkeep and so on are all under severe scrutiny and a policy of not replacing any of the 1,200 or so staff who leave has been adopted. The Daresbury laboratory, which houses the 5 GeV electron synchrotron 'Nina', has had its budget cut back from about £4 million in 1970 to about £3.7 million in 1971 and is also planning to reduce its staff of 545 by about 5 per cent by March 1972—again by adopting a policy involving no redundancies. The question of which accelerator, if either, is ultimately to be shut down must await the outcome of the deliberations of the Science Research Council, but what is clear is that at least one machine must be maintained to ensure that particle physicists who are not involved at CERN during the next decade or so and are keeping up the teaching of high energy physics in British universities have a national experimental facility.

In France there are similar belt-tightening operations in progress and the Commissariat à l'Energie Atomique has suffered a cut in budget from 1,827 million French francs in 1970 to 1,720 million francs in 1971. The CEA does, however, hope to make up the deficit from the production and sale of nuclear materials. On the other hand, the Centre Nationale de la Recherche Scientifique (approximately equivalent to the SRC) has had its budget enlarged from 216 million to 252 million francs. The overall situation is more difficult to assess because the French contribution to the CERN budget is an expense of the French Foreign Office, but it does not seem that the cancellation of any project in hand at present is likely. The demise of the plan for a French national accelerator was, however, probably occasioned chiefly by the prospect of additional French contributions to the CERN budget.

Original Contributions

What, then, does the future hold for the national high energy laboratories of the CERN member states? As CERN becomes increasingly the centre of gravity of the high energy physics effort in Europe, so there will be a tendency for the national laboratories to be used more and more for the development and testing of sophisticated equipment to be used either in the experiments carried out at CERN or in the further development of the accelerator itself. Much of the technology which may eventually enable superconducting magnets to increase the energy of the CERN accelerator to 1,300 GeV has been developed at the Rutherford laboratory, for example, and it seems reasonable that this laboratory should continue to shoulder a significant part of the developmental responsibility. What is important is that all the national laboratories should continue to possess their own identity in the sense that they are not entirely reliant on work immediately relevant to CERN and can pursue their own lines of research and make original contributions to the future development of European high energy physics.

¹ Ramm, C. A., *Nature*, **227**, 1323 (1970).

² Ramm, C. A., *Nature*, **230**, 145 (1971).

NUCLEAR ENERGY

Signs of Change in Euratom

Euratom has failed to develop a unified European nuclear policy because it has been dominated by political considerations. A change in its research planning could, however, smooth the path to a more hopeful future.

EURATOM cannot, by any stretch of the imagination, be considered a shining example of European cooperation in science and technology. Like many other European agencies, its aims have been frustrated by industrial nationalism among the six member countries of the European Economic Community, and it has patently failed in its attempt to develop a unified European nuclear industry. The problems that have dogged the organization during its thirteen year lifetime are depressingly familiar. National governments, concerned that they are not receiving sufficient economic returns from investments in multinational research efforts, have travelled their own nuclear roads and there has been duplication of effort throughout Europe; squabbles have broken out in the Euratom Commission, budgets have been withheld and there have even been rumours that the organization would be wound up. Like European space research, Euratom has always been something of a political football, and technological decisions have been subjected to national political considerations. But there are signs at last that Euratom may be laying the foundations for a slightly more rosy future.

Established in 1957 with the signing of the Treaty of Rome, Euratom started life in the heady atmosphere which surrounded nuclear energy in the late 1950s. The UK had then just announced plans for its first nuclear power programme based on magnox reactors with a total capacity of 5,000 MW, the US was about to embark on the development of a large network of privately owned nuclear power stations with support and encouragement from the US Atomic Energy Commission, and it was then forecast that installed nuclear capacity in the EEC would reach 17,000 MW by 1970, under the careful guidance of Euratom. The immediate task of the new agency was therefore to initiate a vast research programme which would help the member countries to develop their nuclear potential without duplication of research effort. Even in 1963 Heinz Krekler, a member of the Euratom Commission, said in a speech in London that he was concerned to tell his audience "how Euratom is welding the community's atomic resources into a single coordinated and harmonized effort".

By 1967, however, the ambitious forecasts of ten years before were drastically revised downwards. Euratom then estimated that installed capacity in the six member countries would reach only 4,000 MW in 1970, 17,000 in 1975 and 60,000 in 1980. In the event, however, only slightly more than 3,000 MW was installed by 1970 and it now seems likely that the estimated capacity in 1975 will reach no more than 12,000 MW. The blame for this cannot, of course, be laid squarely at the door of Euratom, for developments in the EEC are dogged by the same problems that have beset the USAEC and the UKAEA. Teething troubles with reactors, doubts about the best form of second generation reactors and concern about environmental aspects of nuclear power stations have all combined to dampen enthusiasm for nuclear power generation. But as for Euratom, perhaps the largest thorn in the flesh of the governments of the member countries has been the organization of the agency's research effort. This concern finally culminated in the withholding of the Euratom draft research and investment budget by the European Parliament in November 1970, until research was reorganized. The reason given was that research planning then in use was likely "to prolong the

present stagnation and absence of decisions in the structural field". Happily, however, plans for reorganization suggested by the commission of the EEC last October were approved by the Council of Ministers in December. Euratom's research activities may now be placed on a more viable footing.

The changes are geared into a restructuring of the way that joint research and development activity is planned and controlled within the EEC. But for Euratom the changes are in many respects reminiscent of the restructuring that has taken place in the UKAEA—Euratom's Joint Research Centre will now be more autonomous, and it will undertake more research on a contract basis in non-nuclear fields. This move, it is hoped, will open up possibilities for private industry in the EEC to place research and development contracts with a European agency with the result that there should be less duplication of effort among the European countries.

The plan is essentially one of bureaucratic reorganization which will take much of the administrative work concerned with planning research from the shoulders of the Commission of the EEC and place it in the hands of a high level policy-making committee composed of officials responsible for research policy in the member countries. This committee will have an executive agency to implement its decisions. Called the European Research and Development Agency, it will come under the umbrella of the commission, but the important point is that it will have some control over the funds made available to the community for scientific research, and it will therefore have some degree of autonomy. It is hoped that this structure will make for more flexible planning which can be geared more closely to the needs of the member countries, and that it can be used to bypass the confusing and largely uncoordinated jungle of committees that has been concerned with research planning in the EEC in the past.

This move towards autonomy is also evident in the changes that have been made in the Joint Research Centre of Euratom. In the past, the research programme of the centre, which is in effect four separate institutions situated at Ispra in Italy, Petten in the Netherlands, Geel in Belgium and Karlsruhe in Germany, has been planned from year to year. The upshot of this was that long-term expensive projects tended to be carried out by member countries themselves, and the only real value of the Joint Research Centre was that it provided some specialized facilities that were not available elsewhere. It is now intended, however, that the centre should concern itself with work that is not necessarily confined to the nuclear field, the chief criterion, in the words of the EEC Commission, being that it undertakes work "in fields where the research effort of member states is insufficient".

That is certainly a move in the right direction, but it may have come too late to change the situation in which individual member countries are each carrying out their own, largely uncoordinated research into the development of nuclear technology. Where the new centre may play an important role, however, is in the field of research into the environmental aspects of nuclear power generation—concern for the environment has already resulted in the postponement of a power station at Ludwigshafen in Germany and so-called thermal pollution could well curb

the development of nuclear power along the banks of the Rhine.

One factor which, of course, could have a potentially large effect on Euratom is the entry of Britain into the EEC. But, although Mr Nicholas Ridley, Under-Secretary of State at the Department of Trade and Industry, has assured the House of Commons that Britain would play a full part in the work of Euratom if entry into the EEC were secured, it seems doubtful that participation would involve more than exchange of information. Rumours that Winfrith would be turned into a Euratom establishment were current last time Britain applied for membership of the EEC but

are certainly not in evidence now, and the chief effect on the British nuclear industry will probably be the removal of tariffs on the products of the British Nuclear Fuels company.

The seeds of change that have been sown in Euratom will probably not be sufficient by themselves radically to alter the position of nuclear energy in Europe, but they are at least signs of hope. The organization will only be made to function efficiently, however, if it is not simply a political pawn of the member countries. The same remark, of course, could be made about almost every European scientific agency.

SPAIN

Scope for Improvement

The imminent third development plan should help to link scientific research much more strongly to technological development. During the next decade the introduction of an integrated educational system could significantly relieve the present shortage of trained scientists.

SCIENTIFIC and technical research in Spain is carried out partly in official centres dedicated entirely to this end and partly in laboratories belonging to industrial firms, in universities and higher technical schools, in research associations formed by firms with similar interests, and as contributions to international projects by means of contracts or participation in the work of international bodies.

Spain has about two hundred official research centres, which belong to fourteen different ministerial departments. The most important grouping is the Consejo Superior de Investigaciones Científicas (Higher Council for Scientific Research, belonging to the Ministry of Education and Science), which consists of 184 centres, of which 86 cover the fields of science and technology and 98 those of the humanities and social sciences. Ninety-six of these centres belong entirely to the CSIC and the remainder are centres coordinated with university departments and other bodies, or centres receiving some financial support from the CSIC.

The next largest groupings are the Spanish Nuclear Authority (JEN), the National Institute of Aerospace Technology (INTA), the National Institute of Agricultural Research, the Geological and Mining Research Institute, the Centre for Studies and Experimentation in Public Works, the Forest Research and Experimentation Institute, the Division of Animal Biology, the Geographical and Cadastral Institute and the Spanish Institute of Oceanography. Scientific research for defence purposes is carried out in separate centres, and some of these also provide a certain amount of technical assistance to the armed forces.

The size of the official research centres is generally very small, and in fact only two of them employ more than 250 scientists and engineers; seven employ between 50 and 250, forty-seven employ between 10 and 50. The remaining 145 centres have less than 10 graduate staff each and are nearly all dedicated to the humanities and social sciences.

Companies and Universities

Research in industrial companies is carried out in about three hundred laboratories of various types, of which only six employ a staff of the order of 50 scientists and engineers; each of the rest have less than 20 graduate staff. The data are unfortunately not very reliable, because of the large number of small firms.

It is very difficult to assess the volume of university research because of the large number of divisions and the sharing of the research activity with that of teaching. In October 1968, there were sixteen universities which employed a total of 6,700 academic staff.

During the academic year 1966 to 1967 there were 141,400 students registered in Spanish universities and the average student/teacher ratio was about 20—an average value for a very unequal distribution.

The evergrowing size of the universities creates an urgent need for a properly financed national plan for the training of university teachers of adequate quality and in sufficient numbers. It does not seem feasible for the universities to achieve this end without the cooperation of the research centres and in this sense the proposed creation of a Graduate Institute of Molecular Biology, which would be a cooperative venture between the new University of Madrid and the CSIC, could be a useful experiment in the training of post-graduate students for careers in higher education and research.

The total number of scientists and technologists engaged in full-time research in Spain is about 3,800, of which 1,300 are in industry and 2,500 in official and higher education centres. Eleven hundred of the last of these are chiefly occupied in applied research and development and 1,400 are principally concerned with fundamental research. The total figure represents about 13 research workers for every 100,000 inhabitants, which is equivalent to only 2 per cent of the total number of scientists and engineers in the country.

It is estimated that Spain spends 4,300 million pesetas (£25 million) annually on research, of which 1,500 million are provided by industry, 350 million are spent in higher education establishments and 2,450 million in official centres.

The Distribution of Staff in Spanish Universities

Catedráticos (full professors)	923
Persons in charge of vacant chairs	112
Agregados (readers)	211
Adjuntos (lecturers or assistant professors)	1,749
Teachers in charge of courses (chiefly adjuntos)	1,708
Ayudantes (demonstrators, mostly postgraduate research students)	2,000

This represents only 0.23 per cent of the gross national product. Twenty-two per cent of the total is spent on fundamental research, 43 per cent on applied research and 35 per cent on technological development.

Within the government there is a Committee for Science Policy which receives recommendations from an Advisory Committee for Scientific and Technological Research. The Economic and Social Development Plan has an Office for Scientific Research and Technological Development, charged with the planning of these activities and their review as the plan is put into practice. The coordination of defence research is achieved through an inter-ministerial committee. Between these higher bodies and the executive organizations there are a number of coordinating bodies.

The structure of research in Spain does not differ much from that in most of the developing or developed countries, except perhaps in the non-existence of a high level permanent executive body embracing all research activity. Nevertheless the figures given reveal a significant difference in comparison with those of other European countries.

Three important problems, economic resources, personnel, and administration, can be identified. The first is obviously related to the overall policy adopted in allocating the country's resources, but the problem of personnel (low salaries, inadequate status and numerical shortage) is related to the interest at all levels in science and technology. There is a need to outline some clear prospects in order to attract better qualified young people to a research career.

The problem of technological development is really a combination of several factors—the limited interest in research shown by industry, the very small size of most industrial companies and the lack of information about what the relevant research centres can offer them, the drift of some research sectors towards problems which (posed by industry) are more routine testing than creative research, and the discrimination of the consumer in favour of foreign brand names even though the products are made in Spain. Finally, of course, the introduction of foreign capital has inhibited "homebred" research and encouraged the purchase of foreign expertise.

Future Prospects

The changes which have been taking place in the last few years are important. The industrialists are beginning to regard research as a tool for increasing the competitiveness of their products, and not only are the government and legislative body becoming aware of the importance of these problems but the climate of public opinion is also more favourable.

The advent of the third development plan (1972 to 1975) has meant the conversion of the old Commission for Scientific and Technological Research, which during the first two plans regarded research as just another factor in the situation, into the Office for Scientific Research and Technological Development. This change is significant and has two important aspects; on the one hand it links research firmly with technological development and, on the other, it aims to promote a strong horizontal interaction between all sectors of production and their development during the next decade. This structural modification also embodies a clear change in outlook, and stems from a new type of planning for scientific research and technological development in harmony with the strategy for the overall development of the country up to 1975.

Thus the fundamental principle which is to underlie the policy for scientific research and technological development in the third plan is that of competitiveness linked to a rigorous selectivity—alongside the competition specified in the guidelines which affect the sectors of production. This policy has consequently been implemented in the selection of a certain number of sectorial plans and a small number of large programmes.

Fundamental research, which cannot be expected to show a financial profit within a fixed time scale, will be supported not on the basis of choosing the most technically attractive projects, but by taking account of the prestige of scientific groups who have proved their ability in the past. To back up this policy, the training of research personnel will be increased, and emphasis will be placed on establishing optimum conditions for the functioning of the official research centres.

At the moment, fundamental research in Spain presents a very uneven picture. Biological research is carried out almost exclusively in the CSIC centres and in the university faculties of science, medicine and pharmacy. One can see definite and respectable developments in the field of biochemistry, but other basic fields in fundamental biological research (such as the chemistry and physical chemistry of biological macromolecules) are less well developed, as are other disciplines of medical importance such as virology, microbiology and immunology.

Chemistry research is, generally speaking, the sector which has been most pursued in Spain. Organic chemistry has been applied to the study of natural products of the Spanish flora, and to the synthesis of products of biological and pharmacological activity. Research on polymers and on free radicals has also acquired some importance. In contrast, extensive gaps are evident in the field of inorganic chemistry, and the most important work has been done on silicates, because of the implications for agricultural and geological sciences which are of great interest to the country as a whole.

The developments in the chemical sciences have, in turn, directed physical chemistry research towards the study of reaction mechanisms and structural problems of small molecules and biomacromolecules. But research of a more physical or theoretical nature into these problems has shown only limited growth.

In many ways research in physics and mathematics is even more patchy. The physics of high and low energy particles is perhaps the most developed field in Spain and one important joint scientific group has been formed recently by the new University of Madrid and the CSIC to specialize in solid state physics and high energy theoretical physics.

As mentioned above, it is probably true to say that the chief problem hindering the development of scientific research in Spain is that of insufficient funds to train and support research staff. Over the next decade the total budget of the Ministry of Education and Science should rise from its 1972 estimate of 40,265 million pesetas (some 2.1 per cent of the gross national product) to about 94,000 million in 1981. This large increase is principally to finance the educational reforms enshrined in the general law of education passed in July 1970; at present only 3.7 per cent of the ministry's budget is spent on research. This law incidentally is the first ever in Spain to plan an integrated educational system from primary to postgraduate level, and as such was much needed, but it will probably require more than the ten years envisaged for its full implementation. During this time developments in educational thinking will no doubt modify it considerably.

Another factor in the personnel problem is that Spanish industry still offers better opportunities to the bright young graduate than the lower rungs of the university ladder, although university salaries are now becoming competitive with those offered by industry. This, coupled with a universally heavy teaching load, is the most important impediment to the development of research in the university sector.

All in all, however, the prospects for Spanish science are bright, and the opportunities for interchange of scientists and scientific information with other countries (of vital importance to the development of Spanish research) are better than ever.

HUNGARY

New Centre for Biologists

Parts of a new centre for biological sciences have recently been opened in Szeged. The problem of the lack of contact with other biologists throughout the world may be somewhat alleviated by the encouragement to be given to foreign scientists to work at the centre.

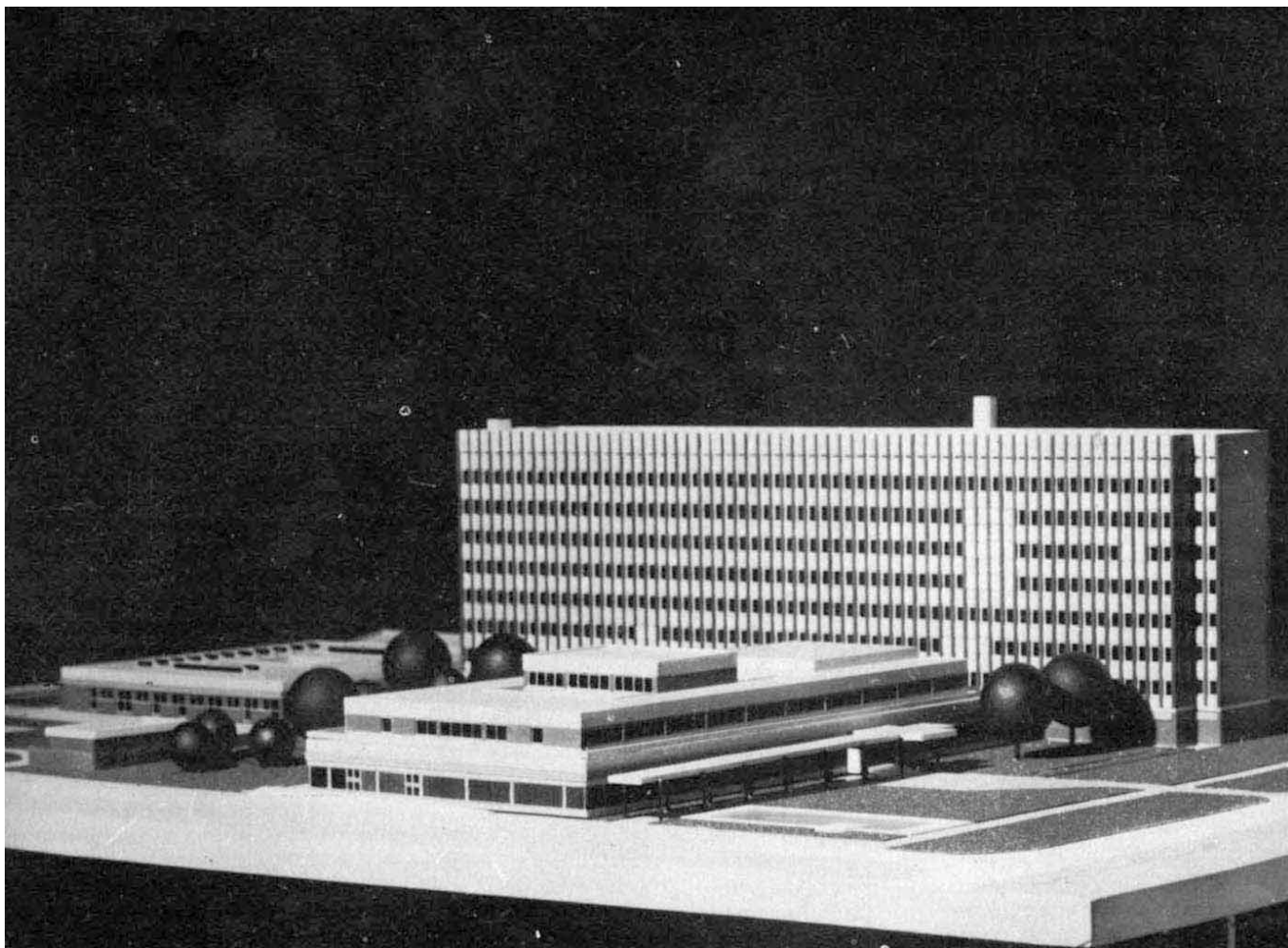
A COMPLEX of laboratories for biological research is in course of construction at Szeged, Hungary. The building is being financed by the Hungarian Academy of Sciences and half of it has already been completed and is in use; the remainder is expected to be finished by the end of 1973. The eight story laboratory building is more than one hundred metres long and workshops, offices, library, auditorium and guest rooms are to be housed in adjoining buildings. When complete, the buildings will be occupied by about 150 scientists and 350 other staff; at present only about half this number are working there.

Four institutions of the Academy of Sciences will eventually make use of the new facilities which are under the direction of Academician F. B. Straub—these are the institutions specializing in biochemistry, biophysics, genetics and plant physiology. There are plans for several research topics of a rather interdisciplinary nature which include regulation of gene activity, genetics in tissue culture, the role of mem-

branes in neurobiology, the evolution of mitochondria and the senescence and virus resistance of plant leaves.

Szeged already has a well established university, and medical and biological education figure prominently in its curriculum. It is the intention of the Hungarian academy that its biological institutions should have close ties with the universities and this is no less applicable in the case of Szeged. The ultimate aim of most biological research in Hungary is to provide benefits and new ideas for Hungarian agriculture and the food and pharmaceutical industries.

Hungarian biologists are well aware of the value and necessity of international cooperation and there are plans afoot to make the facilities at Szeged available to both foreign students and to foreign visiting scientists. This move should help to keep Hungarian biologists in closer touch with experimentalists in other parts of the world and should go some way to making up for the lack of foreign books and journals.



Model of the new biological research centre at Szeged.

Release of Noradrenaline from Sympathetic Endings

J. H. BURN

Emeritus Professor of Pharmacology, Oxford University

There is evidence that in the transmission of certain nervous impulses, acetylcholine stimulates the release of noradrenaline which then acts as the transmitter substance.

WHEN he showed that the transmission of nerve impulses from the ends of sympathetic postganglionic fibres was due to the release of noradrenaline, von Euler was expounding an exception to the rule that at all motor terminations the transmitter was acetylcholine. Everyone assumed that the nerve impulse passing down the postganglionic fibre released noradrenaline directly. This assumption was made in spite of two facts. First, stimulation of many sympathetic postganglionic fibres in various animals had been shown to release acetylcholine as well as noradrenaline¹⁻⁸. Nevertheless, most physiologists and pharmacologists ignored the evidence of this release of acetylcholine. Second, the cells of the adrenal medulla, where the release of adrenaline and noradrenaline is effected by acetylcholine, are derived from the embryonic sympathetic nervous system.

In 1959 Rand and I suggested that the acetylcholine released by sympathetic stimulation might be concerned in the release of noradrenaline. In support of this we performed experiments in which we applied electrodes to the sympathetic postganglionic fibres running to perfused vessels of rabbit ear. Stimulation then caused vasoconstriction due to the release of noradrenaline. The vasoconstriction was greatly increased when physostigmine was added to the perfusion fluid⁹. Physostigmine would not affect the vasoconstriction if the noradrenaline was released directly by the nerve impulse, but would do so if the nerve impulse first released acetylcholine, and if this in turn, acting as a "cholinergic link", then released the noradrenaline. Acetylcholine is rapidly destroyed by cholinesterase, and the physostigmine, by inhibiting the cholinesterase, would increase the effective concentration of acetylcholine, and consequently the amount of noradrenaline released.

Histological and Pharmacological Evidence

Until recently there has been no histological evidence to support this concept, but the position has now changed. Waterson, Hume and de la Lande¹⁰, extending the earlier work of Grant and Thompson¹¹, cut transverse sections through the central artery of the rabbit ear, and found that acetylcholinesterase and butyrylcholinesterase were present at the site where noradrenaline was also present in the nerve terminal network, that is next to the medial-adventitial border. The enzymes disappeared 21 days after removal of the superior cervical ganglion, as did also the noradrenaline, but the enzymes were unaffected by removal of the noradrenaline with reserpine. Thus clear evidence was provided of a cholinergic element in close relation to an adrenergic element, both being sympathetic in origin.

Similar but much more informative results have been obtained by Eränkö, Rechartt, Eränkö and Cunningham¹². The innervation of the pineal gland is also entirely sympathetic,

and comes from the superior cervical ganglion. They examined the rat pineal, by two methods: fluorescence, which enables the precise location of noradrenaline to be defined, and the thiocholine method, which marks the site of acetylcholinesterase (when a specific inhibitor for butyrylcholinesterase is present). The presence of acetylcholinesterase has always been found to indicate the proximity of acetylcholine. Eränkö and his colleagues showed that fluorescent and acetylcholinesterase-positive nerve nets formed identical patterns, and that bilateral removal of the superior cervical ganglia resulted in complete disappearance of fluorescence and acetylcholinesterase from nerve fibres. Electron microscopy showed also that the acetylcholinesterase was located either in the membrane or just outside the membrane of an axon terminal containing small

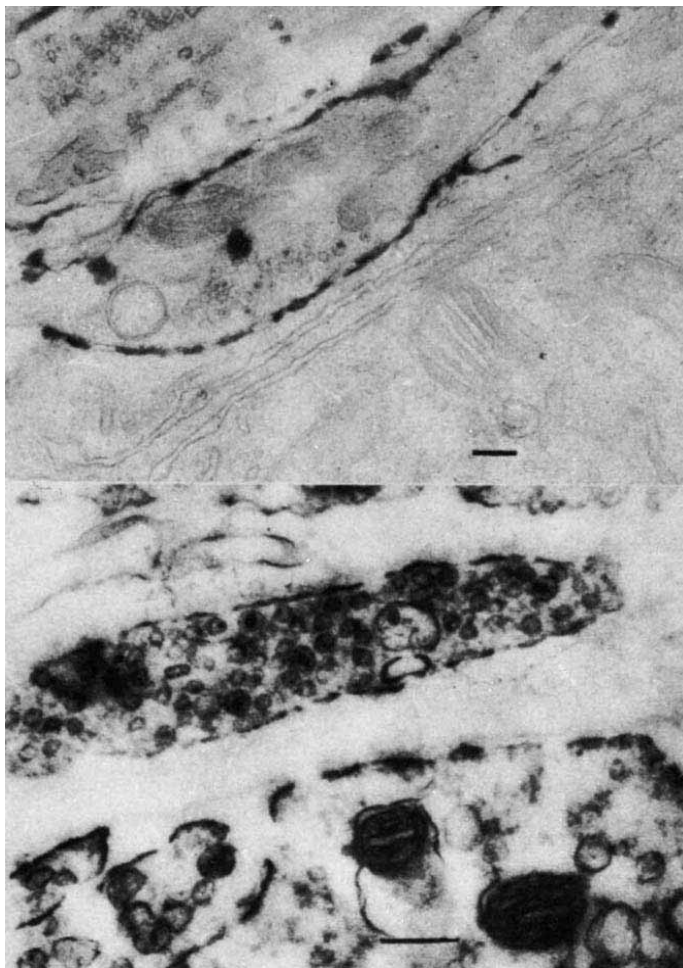


Fig. 1 Electron microscope pictures prepared by Eränkö, Rechartt, Eränkö and Cunningham¹², of adrenergic terminals in the rat pineal gland. The tissue was stained for acetylcholinesterase in the presence of tetra-isopropylpyrophosphoramide (iso-OMPA) to inhibit butyrylcholinesterase. In the upper picture the reaction product shows acetylcholinesterase immediately outside the terminal membrane, and in the lower picture within the terminal membrane itself. The terminal contains small granular vesicles. The bar is 2000 Å. See text for details.

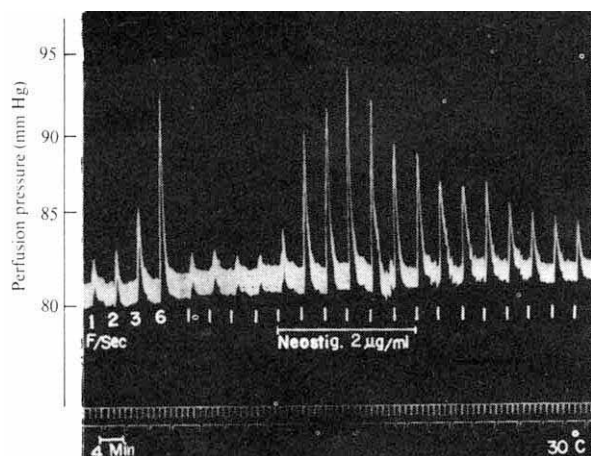


Fig. 2 Perfusion of the rat's superior mesenteric artery and its branches with Tyrode solution at 30° C. The record shows the changes of pressure in the arterial cannula when postganglionic sympathetic fibres were stimulated at the frequencies given below the record. Neostigmine was added to the perfusion fluid for the period shown, when stimulation was at 1/s. (From ref. 19.)

granular vesicles (Fig. 1). This is the first picture of an adrenergic terminal, which releases noradrenaline as the principal transmitter, where staining for acetylcholinesterase has also been carried out. The picture suggests that the nerve impulse, arriving at the terminal, may release acetylcholine from a store outside (or within) the membrane which then makes the membrane permeable to calcium.

The crucial evidence for the part played by acetylcholine in releasing noradrenaline has come from the use of anticholinesterases. In addition to the preliminary results described already, there have been demonstrations in the dog femoral artery¹³, the nictitating membrane¹⁴, the isolated heart¹⁵, the taenia coli of the guinea-pig¹⁶, the retractor penis¹⁷ and the renal artery¹⁸ of the dog. Without exception these showed that physostigmine and other anticholinesterases including diisopropylfluorophosphate (DFP) increased the response to electrical stimulation of postganglionic fibres when applied at frequencies less than 6/s.

Striking results have, however, been obtained by Malik¹⁹, who perfused the superior mesenteric artery of the rat and its branches, and recorded the increase of pressure in the arterial cannula when the vessels constricted in response to postganglionic stimulation²⁰. An example is shown in Fig. 2, in which maximal stimulation at intervals of 4 min at the frequencies shown caused increases of pressure. The first four stimulations were given at frequencies increasing from 1 to 6/s. Then, when stimulating at a rate of 1/s, the addition of neostigmine (2 µg/ml.) to the perfusion fluid caused an increase in the response which reached a peak in 12 min, and then declined. At the peak, stimulation at a rate of 1/s produced a response equal to that produced by a rate of 6/s before neostigmine was added.

This increase in response is explained by the inhibition of cholinesterase by neostigmine, with a consequent rise of acetylcholine concentration and release of more noradrenaline. This interpretation is confirmed by the results of Malik and Ling²¹, who showed that at 22° C, the addition of acetylcholine itself in the very low concentration of 50 pg/ml. steadily increased the response to stimulation as long as the acetylcholine was present in the perfusion fluid (Fig. 3). A similar effect was obtained when a higher concentration of acetylcholine (2 ng/ml.) was allowed to act for the very short time of 15 s (Fig. 4). Thus in these conditions acetylcholine in the perfusion fluid added its action to that of the acetylcholine released from the nerve by stimulation, so that more noradrenaline was released than by stimulation alone.

Decline from the Peak

As Fig. 2 shows, the addition of neostigmine to the perfusion fluid caused an increase in the response for the first 12 min, but thereafter the response declined. This is probably explained by accumulation of excess acetylcholine, resulting in the occupation of nearly all free receptors. The impulse passing down the nerve fibre may release acetylcholine, but if there are no unoccupied receptors, then this acetylcholine is without effect, and its action is said to be blocked. Such a failure was demonstrated by Malik and Ling. In Fig. 5, acetylcholine in a concentration of 5 ng/ml., added to the perfusion fluid, not for 15 s but for 20 min, greatly reduced the response to stimulation. This reduction disappeared as soon as the acetylcholine was removed, to be followed for a time by a greater response than the initial response.

As well as with neostigmine, experiments were carried out with physostigmine and with DFP, each tested in five to ten experiments at four frequencies of stimulation. Fig. 6 shows the effect of DFP tested at a frequency of 1/s. The mean result at each frequency is plotted in Fig. 7, which shows that the mean percentage increase in the response to stimulation was greatest

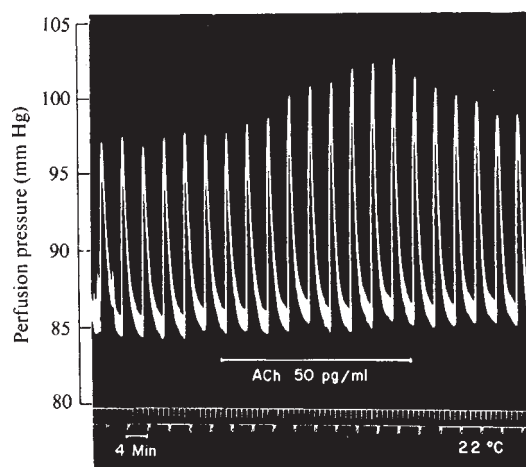


Fig. 3 Perfusion as in Fig. 2, but carried out at 22° C, at which temperature cholinesterase acts very slowly. The addition of acetylcholine, 50 pg/ml., to the perfusion fluid gradually increased the response to stimulation at 7/s. The increase continued while acetylcholine was added. (From ref. 21.)

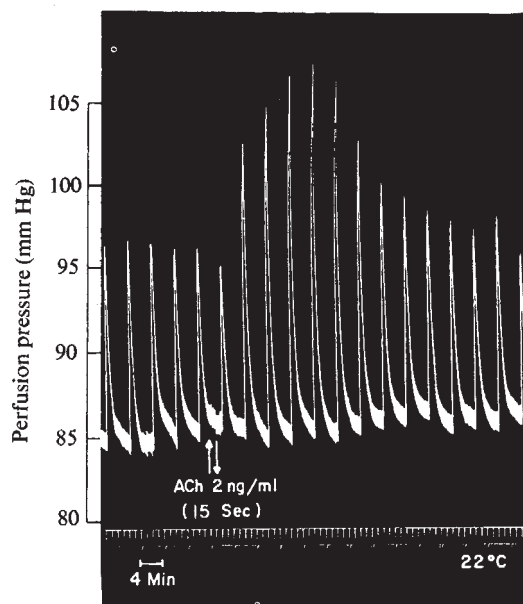


Fig. 4 Perfusion as in Fig. 3, showing the effect of adding acetylcholine, 2 ng/ml., to the perfusion fluid for the brief period of 15 s. The increase was much larger. (From ref. 21.)

at the lowest frequency, and declined as the frequency increased until at 6/s there was no appreciable increase. At the lowest frequency cholinesterase had the longest time in which to destroy acetylcholine between two stimuli, and therefore an anticholinesterase had the greatest effect. The presence of cholinesterase clearly has a great influence on the amount of noradrenaline released in the frequency range up to 6/s, and it is interesting that Folkow²² found that the normal physiological range did not exceed 8/s. Several workers who failed to find any effect of anticholinesterases on the release of noradrenaline used frequencies of 6/s or higher.

It is evident that in the conditions described, the release of noradrenaline is accomplished by acetylcholine. Following Douglas and Rubin²³, Gibbons and I showed that the release of noradrenaline, whether in response to stimulation²⁴ or to acetylcholine²⁵, was dependent on the concentration of calcium; we concluded that, as when acetylcholine acts on the membrane of the chromaffin cell in the adrenal medulla, so acetylcholine released by stimulation of the postganglionic fibre made the membrane surrounding the store of noradrenaline permeable to calcium. Calcium then entered and released noradrenaline.

Adrenergic Blocking Agents

There are substances in clinical use for the reduction of high blood pressure, such as guanethidine, the action of which is explained by that of acetylcholine shown in Fig. 5, in which continuous infusion for 20 min greatly reduced the response to stimulation. The first of these "adrenergic blocking agents" was a derivative of choline, xylocholine. Then came bretylium, related to choline in being a derivative of dimethylammonium, as is also guanethidine. Although it is not obvious, Rand and Wilson²⁶ have shown that there is a close similarity between the molecular structures of guanethidine and xylocholine. Both bretylium and guanethidine resemble acetylcholine in being able, in a single large dose, to increase the rate of beating of isolated rabbit auricles. Since they do not have this action on atria from rabbits treated with reserpine, the increase must be due to the release of noradrenaline.

There is thus a fundamental similarity between acetylcholine, bretylium and guanethidine. All three can release noradrenaline

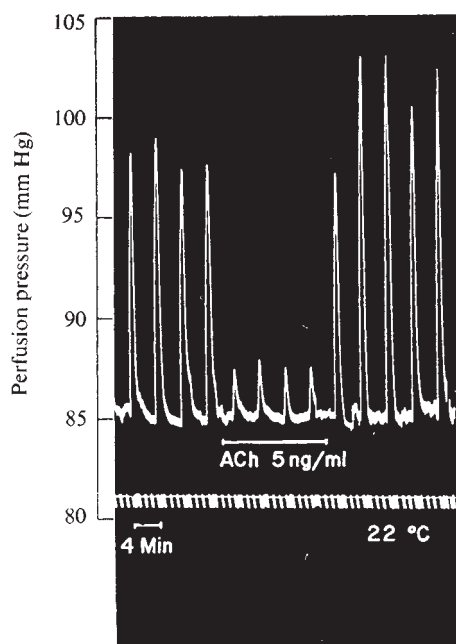


Fig. 5 Perfusion as in Figs. 3 and 4. The addition of acetylcholine, 5 ng/ml., to the perfusion fluid for 20 min reduced the response to stimulation at 7/s. When acetylcholine was removed, the responses became for a time greater than the initial responses. (From ref. 21.)

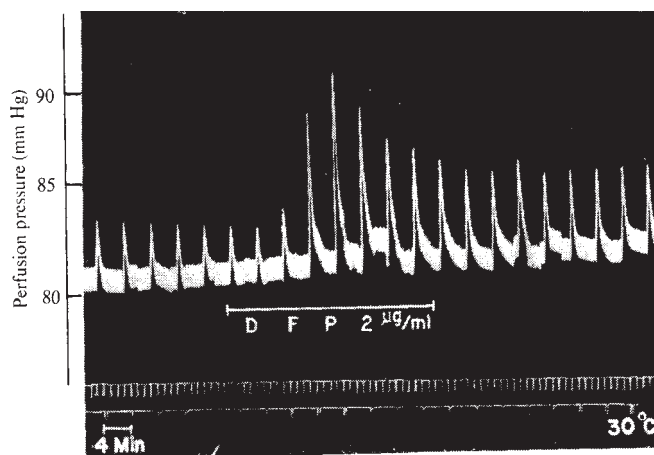


Fig. 6 Perfusion as in Fig. 2, showing the increase in the response to stimulation at 1/s when DFP (2 µg/ml.) was added to the perfusion fluid. (From ref. 20.)

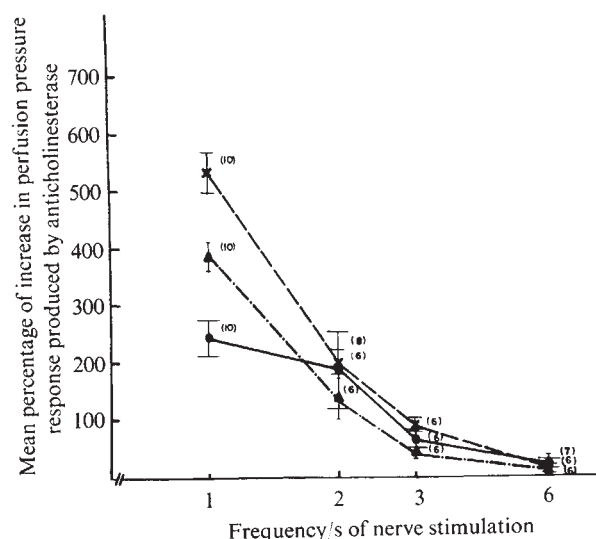


Fig. 7 The effect of frequency of stimulation (abscissa) on the mean percentage increase in the response (ordinate) caused by (x) neostigmine, 2 µg/ml., (Δ) DFP, 2 µg/ml., and (●) physostigmine (6 µg/ml.). The number in brackets beside each point is the number of experiments from which the mean was calculated. The effect of the anticholinesterase diminished as the frequency rose, until at 5 s there was no increase. (From ref. 20.)

from sympathetic endings and all three can block its release. But in the body, the situation never arises in which acetylcholine, liberated by sympathetic impulses, causes a block. It can only do so when it is infused in a constant stream so that it leaves few receptors free. The situation for bretylium and guanethidine is the reverse of this. When introduced into the body in a single dose, they become attached to the same receptors and, once attached, they are dislodged very slowly. Many receptors are blocked, and sympathetic tone falls.

There are two further points of similarity between the three substances. The block produced by each of them is diminished or abolished when the concentration of calcium is increased²⁷, which implies that a block is always relative to a particular gradient of calcium ions from outside to inside, and that the block is insufficient to prevent the entry of calcium if the external concentration is raised.

The second point of similarity is that the block due to each of the three substances is diminished or abolished by the addition of d-amphetamine to the fluid perfusing the mesenteric arteries²¹. The block due to bretylium and guanethidine, however, is not removed by atropine as is that due to acetylcholine. It should be noted here that dimethyl-phenyl-piperazinium

(DMPP) is very similar to acetylcholine in its action, in that it will produce an effect like that illustrated in Fig. 4, as well as that in Fig. 5, if a concentration of 0.3 µg/ml. (instead of 5 ng/ml.) is used. The block due to DMPP is not affected by atropine, although it is removed by increasing the concentration of calcium or by adding d-amphetamine²⁸.

There is evidence that in no circumstances is noradrenaline released by the nerve impulse acting directly. This has been provided by work on botulinum toxin and by the hemicholinium known as HC-3. Botulinum toxin prevents the release of acetylcholine from the terminations of the motor nerves in the diaphragm²⁹. Rand and Whaler³⁰ found that this toxin, injected intradermally at the base of a tuft of hair, blocked the response of the pilomotor muscles of the cat's tail to sympathetic stimulation. When 18 h later the cat was anaesthetized, and the sacral sympathetic trunk was stimulated, the tufts treated with toxin failed to respond, while the other tufts were fully erected. Rand and Whaler also used the Finkleman preparation of a loop of rabbit intestine. They found, in seventeen out of nineteen experiments, that when botulinum toxin (10,000 MLD/ml.) was added to the organ bath, sympathetic stimulation became ineffective in inhibiting the intestinal movements after 4.5 h, although in a control loop it was still fully effective.

Vincenzi³¹ stimulated an excised portion of the atria of rabbits containing the sino-auricular node, using generalized shocks applied to the whole tissue; these caused inhibition followed by acceleration. When he applied botulinum toxin, acceleration was not abolished. Blinks³², however, has shown that the sympathetic effect of field stimulation is still pronounced after 2 h of exposure to 10⁻⁴ M bretylium, concluding that the mechanism of release by field stimulation must be different from that by the nerve impulse.

HC-3 is one of the hemicholiniums which was shown to prevent the synthesis of acetylcholine by competing with choline in its formation. Rand and his colleagues found that the response to stimulation of sympathetic postganglionic fibres was blocked by HC-3 in several organs (Table 1). In all these the response was abolished, and was then restored by the addition of choline; the abolition was therefore due to failure of acetylcholine synthesis. The time taken to block was from 4.5 to 5.5 h except for the rabbit ear vessels where it was only 3 h because the vessels were perfused. Other workers have failed to observe an effect probably because they used much shorter periods.

Action of Acetylcholine on Nerves

When it was first suggested that acetylcholine was involved in the release of noradrenaline by the sympathetic impulse, Ferry³⁶ showed that this might not be a direct action. He demonstrated that when acetylcholine was injected into the splenic artery, antidromic impulses passed along the fibres of the splenic nerve, and so he suggested that impulses passing in the opposite direction to the spleen might be responsible for the release of noradrenaline. Similar observations were made on the heart.

Against this conclusion was evidence³⁷⁻³⁹ that bretylium blocked the nerve impulse in lower concentration and earlier than it blocked the action of acetylcholine. Hauesler *et al.*⁴⁰ recorded the asynchronous discharges set up by acetylcholine

in the sympathetic fibres of the perfused cat heart, measuring at the same time the amount of noradrenaline released. They infused acetylcholine at a concentration of 5 × 10⁻⁵ g/ml. which induced asynchronous firing for the whole period of infusion (1 min) and observed an average liberation of 44 ng/min of noradrenaline. A higher concentration of acetylcholine, however, restricted the asynchronous firing to a few seconds, but greatly increased the output of noradrenaline, for example to 346 ng/min. Thus the electrical activity induced by acetylcholine was entirely unrelated to the amount of noradrenaline released.

Work on humoral or chemical transmission, at least in tissues outside the brain, has made little progress since 1936; for example, we know nothing of the transmission of sensory impulses. The work I have described strongly suggests that in one system, the transmission of postganglionic sympathetic impulses, one transmitter is first released, which in turn releases the main transmitter. There may be a similar arrangement in the nigra-striatal tract in the brain which liberates dopamine, and it is not impossible that a similar arrangement also exists in the sensory fibres. Thus the cholinergic link may be present in other fields as well as in the sympathetic system.

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Table 1 Effect of Hemicholinium HC-3 on Sympathetic Postganglionic Stimulation

Organ	Concentration (g/ml.)	Time to abolition of response (min)	Reference
Cat spleen	5 × 10 ⁻⁵	263	33
Cat atria	5 × 10 ⁻⁴	270	34
Rabbit ear vessels	5 × 10 ⁻⁵	180	34
Guinea-pig colon	5 × 10 ⁻⁵	335	35

Further Evidence of Lower Pleistocene Hominids from East Rudolf, North Kenya

R. E. F. LEAKEY

National Museum of Kenya, PO Box 658, Nairobi

The 1970 expedition organized by the National Museum of Kenya to East Rudolf recovered sixteen specimens of hominid which include some postcranial remains. This material adds further support to the view that two distinct hominids—*Australopithecus* and *Homo*—lived contemporaneously in the Lower Pleistocene in this part of East Africa.

DURING June–August 1970, the National Museum of Kenya organized a further expedition to the extensive fossil beds lying to the east of Lake Rudolf. The sixteen specimens collected during this season bring the total collection of hominids from East Rudolf to twenty-three (Table 1). Among the 1970 finds are five specimens of postcranial material. The specimens collected during 1968 and 1969 have been mentioned briefly¹; a more detailed description is in preparation (R. E. F. L., J. M. Mungai and A. Walker).

The hominid and other vertebrate material has been collected from two principal areas which are referred to as Ileret and Koobi Fora (see Fig. 1 in ref. 2). A preliminary report on the geology of the Koobi Fora area has already been published by Behrensmeyer³. During 1970, Miss Behrensmeyer continued geological work in this locality and a geological team from Iowa State University, C. Vondra, G. Johnson and B. Bowen, undertook a preliminary geological investigation of the Ileret area. A joint report on the geology of Ileret and Koobi Fora follows this article². A definite correlation between the deposits of Ileret and Koobi Fora has yet to be confirmed. The faunal evidence shows that the two areas overlap to some extent, but until further work is completed fossil material will be referred to locality sections. Some of the vertebrate material from Koobi Fora and Ileret is described by Dr V. Maglio in an accompanying article³.

Further archaeological work on the area has been undertaken by Dr G. L. Isaac and several new sites have been located. A detailed report on this aspect of the 1970 expedition will be published later.

Hominid Mandibles

The eight mandibles collected in 1970, with the exception of two, KNM-ER-730 and 731, seem to represent variations of one type and are similar to the two mandibles collected in 1968. The most complete of the eight is KNM-ER-729 which was discovered by Dr P. I. Abell. This specimen consists of a robust corpus with P₄ through M₃ on the right side and M₂ and M₃ on the left side (Fig. 1). The left third and fourth premolars have complete crowns but the roots are damaged and so these teeth are separate from the mandible. The left ascending ramus is missing but the right ramus is fairly complete and lacks only the coronoid process and mandibular condyle. Measurements on this specimen are given in Tables 2 and 3. Some measurements of four of the other mandibles are shown in Table 4.

The robust construction of KNM-ER-725, 726, 727, 728, 729 and 733 is evident from Fig. 1 and Tables 2, 3 and 4. When compared with robust australopithecine mandibles from South Africa, the Peninj mandible from Lake Natron in Tanzania⁵ and the specimens from the Omo Valley of Ethiopia^{6–8}, there is a striking uniformity of the overall morphology. The specimens vary but it is likely that these variations are merely individual differences which might be expected within such a sample. In the absence of additional evidence KNM-ER-725, 726, 727, 728, 729 and 733 and the two mandibles collected in 1968 (KNM-ER-403 and 404) are referred to the genus *Australopithecus*. Although the first hominid mandible collected from the Omo Valley was referred to the new genus *Paraustralopithecus* by Arambourg and Coppens^{6,7}, I feel that the enlarged sample from both Omo and East Rudolf provides strong argument in favour of including this original fragment within the genus *Australopithecus* as defined by Tobias⁹.

Table 1 Hominid Specimens from East Rudolf

Year	Locality	Museum accession Nos.	Details of specimens
1968	Koobi Fora	KNM-ER-403	Right mandibular corpus—fragmented dentition
1968	Koobi Fora	KNM-ER-405	Maxilla—very weathered without dentition
1968	Ileret	KNM-ER-404	Right mandibular corpus—fragmented dentition
1968	Ileret	KNM-ER-417	Fragment of parietal
1969	Ileret	KNM-ER-406	Cranium—no dentition
1969	Ileret	KNM-ER-407	Cranium—lacking facial and maxillary region
1969	Koobi Fora	KNM-ER-164	Fragment of parietal
1970	Ileret	KNM-ER-725	Left mandibular corpus—no dentition
1970	Ileret	KNM-ER-726	Left mandibular corpus—no dentition
1970	Ileret	KNM-ER-727	Right mandibular corpus—no dentition
1970	Ileret	KNM-ER-728	Right mandibular corpus—no dentition
1970	Ileret	KNM-ER-729	Mandible with dentition—ascending rami damaged
1970	Koobi Fora	KNM-ER-730	Left mandibular corpus with M ₁ to M ₃ through symphysis to right P ₄
1970	Ileret	KNM-ER-731	Left fragment of mandibular corpus
1970	Ileret	KNM-ER-732	Right side cranium: maxilla, frontal, parietal to mastoid
1970	Ileret	KNM-ER-733	Cranial fragments; maxilla fragment and left mandibular corpus
1970	Koobi Fora	KNM-ER-734	Parietal fragment
1970	Koobi Fora	KNM-ER-736	Left femur—shaft
1970	Koobi Fora	KNM-ER-737	Left femur—shaft
1970	Koobi Fora	KNM-ER-738	Left femur—proximal end and shaft
1970	Ileret	KNM-ER-739	Right humerus—distal end and shaft
1970	Ileret	KNM-ER-740	Left humerus—fragment of distal end
1970	Ileret	KNM-ER-741	Left tibia—proximal end and shaft

The two remaining mandibles (KNM-ER-730 and 731) collected in 1970 are striking in their contrast to the six mandibles I have just discussed. KNM-ER-730 was discovered by Dr Meave Leakey and is the more complete. This specimen consists of the left side of the mandible with M_3 and M_1 extending through the symphysis to the right corpus which is broken at the fourth premolar. The teeth show heavy wear and the alveolar region shows re-absorption which is most advanced in the anterior part of the mandible. In addition to the measurements detailed in Table 5 and the illustrations in Fig. 2, several characters are worth special mention:

(1) The mandible is small. (2) The third molar is fractionally longer than M_2 but the bucco-lingual dimension of the posterior portion of the tooth is reduced. The extremely worn condition of the crowns, as well as the damage to M_1 and M_2 , prevent definitive description and accurate measurements of the dentition. (3) The mandibular arch is distinctly U-shaped, being wide in the anterior or symphyseal region. (4) There seems to be a mental protuberance or "chin", but the conformation of the anterior symphyseal morphology is difficult to interpret because the alveolar bone has been resorbed around the incisors. (5) The digastric fossae are deep and well defined. (6) The large canine roots suggest that the canines were large. The intercanine width makes it very likely that the incisors were also quite large.

It is clear that KNM-ER-730 is unlike the typical robust australopithecine mandible from the same stratigraphic level at East Rudolf. KNM-ER-731 is very similar to 730 and both are to be provisionally referred to the genus *Homo* but until



Fig. 2 Mandible KNM-ER-730.

such time as further specimens become available I shall not try to identify the species.

Cranial Material

In 1970, an almost complete right half of a cranium (KNM-ER-732), a parietal fragment (KNM-ER-734) and associated cranial and mandibular fragments (KNM-ER-733) were collected.

KNM-ER-732 is of particular interest (Fig. 3 and Table 6). The specimen is broken through the sagittal plane and no fragments of the left side were located. The right half is fragmented but sufficiently complete to provide a great deal of information. The teeth have been broken along the alveolar margin and, apart from a portion of the right fourth premolar, no crowns were recovered in spite of excavation and intensive sieving.

On the basis of its morphology I have little doubt that KNM-ER-732 is australopithecine, but in the absence of further material and taking into account the strong possibility of marked sexual dimorphism within the australopithecines, I propose that, for the present, it be referred to the same species (indet) as the other australopithecine material from Rudolf.

Table 2 Measurements on the Mandible KNM-ER-729

	mm
Length of mandible	130
Bimolar width	90
Molar-premolar series length	77
Height of mandibular body at M_3	48
Height of mandibular body at P_4	49
Breadth of mandibular body below M_2	29
Inter-canine width (at alveolar margin)	21

Table 3 Dimensions (in mm) of the Teeth of KNM-ER-729

	Left side		Right side	
	Mesio-distal	Bucco-lingual	Mesio-distal	Bucco-lingual
M_3	21.5	19.0	22.0	19.0
M_2	20.0	18.0	19.5	18.0
M_1	—	—	(16)	15.5
P_4	14.0	15.0	14.0	16.0
P_3	12.0	13.0	—	—
C	(10)	(9)	—	—
I_2	—	—	7.0	(5)
I_1	—	—	—	—

Brackets indicate estimated measurements.

Table 4 Measurements on the Half Mandibles (in mm)

	KNM-ER-725	726	727	733
Breadth of mandibular body below M_2	33	30	26	(29)
Height of mandibular body below M_3	34	42	(29)	(34)

Brackets indicate estimated measurements.

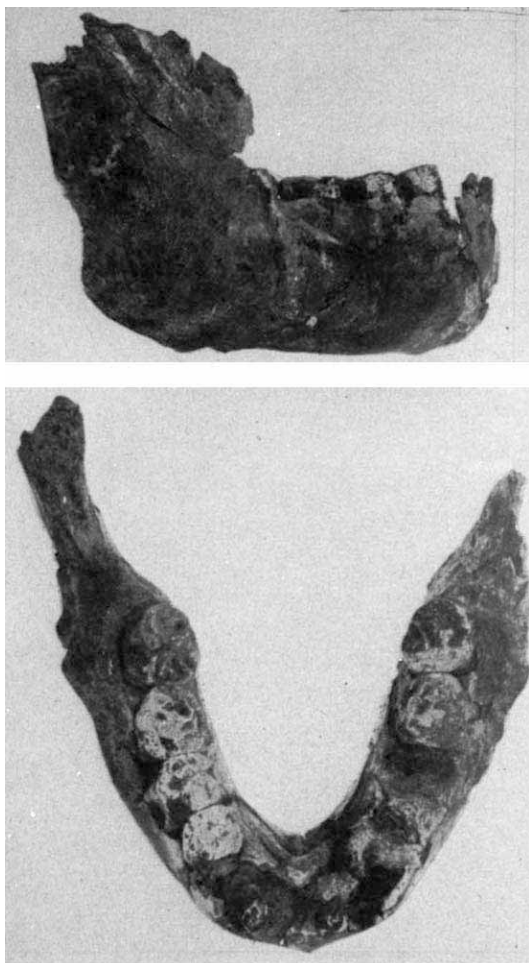


Fig. 1 Mandible KNM-ER-729.



Fig. 3 Cranium KNM-ER-732.

There is little to be said at present about the parietal fragment KNM-ER-734. The associated cranial and mandibular fragments—KNM-ER-733—are significant in representing the first occurrence in the Rudolf basin of a mandible directly associated with maxillary and other cranial fragments. This specimen is robust australopithecine, and for the present it will be assigned to the same species as the other australopithecine material from East Rudolf.

Postcranial Material

The five good specimens have been compared with *Homo sapiens*, *Pan* and *Gorilla* in a provisional manner for this article. (1) Left femoral diaphysis KNM-ER-737. This specimen lacks epiphyses but is otherwise complete and is apparently free from distortion. It is interesting that it

Table 6 Estimated Measurements on the Demi-cranium KNM-ER-732 (in mm)

Width of face (lateral surface of malar to the midline $\times 2$)	114
Post-orbital constriction	61
Height of left orbit	31
Width of left orbit	36
Inter-orbital width	21
Estimated length of molar-premolar line (P^3 to M^3)	56

resembles the *Homo sapiens* femur in several respects. Its features are as follows. (a) The bone is long and narrow and has unusual antero-posterior and slight lateral curvature. (b) The lateral and medial surfaces are directed backwards in approximately equal proportions; the sharp distinct linea aspera provides a posterior border to the shaft which divides equally the proportionately wide postero-lateral and postero-medial surfaces. These meet at an angle of nearly 100° . (c) The linea aspera is a single, sharp ridge. (d) There is no evidence of supra-condylar lines; the linea aspera runs into the smooth posterior surface of the distal region. (e) The gluteal tuberosity is well developed and prominent. (f) The proximal portion of the anterior surface is flat and is wide mesio-laterally. The medial border is sharp and provides a triangular cross-section.

It is not possible to determine the length of the neck of this specimen but its thin outer cortex and general absence of buttressing suggest that it had a fairly short neck.

(2) Left femoral diaphysis KNM-ER-736. This specimen has been broken anteriorly just above the lesser trochanter and distally some distance below the estimated mid-point, and therefore lacks both epiphyses. The diaphysis is markedly robust and there is no evidence of curvature to the shaft. The specimen is quite unlike *Homo*, *Pan* and *Gorilla* material. Its characteristics are as follows. (a) The shaft has an oval cross-section above the linea aspera. The anterior and posterior surfaces are gently curved into the rounded lateral and medial borders. (b) The linea aspera begins some 80 mm below the lesser trochanter and becomes very pronounced distally. The lateral surface is directed backwards and meets the medial surface at an angle of about 75° . (c) The lesser trochanter is broken but seems to have been medially orientated to a marked degree. (d) The outer cortex of bone is thick throughout the diaphysis and X-rays of the proximal region show thickening of the outer cortex which indicates the possibility of a long neck.

(3) Left femoral diaphysis with proximal epiphysis preserved, KNM-ER-738. This specimen consists of just more than half the shaft represented by a large splinter of the medial surface and the proximal region which is complete except for the greater trochanter (Fig. 4). The join between the two pieces shows interlock of irregularities on both the medullary and external surfaces which leave no doubt that this is a true contact. The shaft bears evidence of having been broken and subsequently healed during life and this has resulted in a thickening of the bone in the region above the commencement of the linea aspera. This thickening is most pronounced on the antero-medial aspects. Its features are as follows. (a) The bone is small and gracile. (b) The morphology of the shaft is very similar to that seen on the robust diaphysis KNM-ER-736. (c) The femur head is small and the neck is long by comparison to the proportions observed in *Homo*. (d) The neck is compressed antero-posteriorly and there is an unusual, small tubercle situated on the anterior surface. (e) The linea aspera is prominent and forms a common posterior border to the postero-lateral and postero-medial surfaces which meet at an angle of less than 70° .

Although small, this specimen is not unlike other femoral fragments that have been collected elsewhere and assigned to *Australopithecus*.

(4) Left tibia KNM-ER-741. This specimen consists of the proximal epiphysis and approximately half the length of the

Table 5 Measurements on the Mandible KNM-ER-730 (in mm)

Height of mandibular body at M_3	31.5
Breadth of mandibular body below M_2	19
Estimated breadth of mandible at M_3 (buccal surface of left M_3 to midline of the mandible multiplied by two)	60
Breadth across canines (buccal surface of left canine root to buccal surface of right canine root)	(31.5)
Mesio-distal dimension of M_3	(13.0)
Mesio-distal dimension of M_2	(12.5)
Mesio-distal dimension of M_1	(11.5)
Bucco-lingual dimension of M_3	(11.5)
Bucco-lingual dimension of M_2	(11.5)
Bucco-lingual dimension of M_1	(11.5)

Brackets indicate estimated measurements.

Fig. 4 Femoral fragment KNM-ER-738 and humerus KNM-ER-739.



shaft. Its morphology is of considerable interest and will require detailed examination. Until such time, superficial comparison with extant models is considered to be of little value. The anterior border of the shaft and the tuberosity are mesio-laterally compressed resulting in wide and flat medial and lateral surfaces. The medial condyle is powerfully buttressed. The shaft shows considerable torsion in relation to the transverse axis of the condyles.

(5) Right humerus KNM-ER-739. This specimen is complete except for the proximal epiphysis. The bone is robust and of large size and the muscle attachments are powerfully developed (Fig. 4). Its features are as follows. (a) The bicipital groove is wide and has a sharp anterior lip in the region above the deltoid tuberosity. (b) The shaft exhibits a distinct anterior curvature in the distal region. (c) The medial supracondylar ridge extends proximally to the roughened area for the insertion of latissimus dorsi and can still be traced proximal to this as far as the fracture on the end of the bone. (d) The medial epicondyle is well developed and has a marked medial extension. (e) The lateral border of the shaft is smooth in the upper half but becomes a sharp ridge distally. There is a distinct lateral flaring to the shaft just above the distal epiphysis and this feature is often seen in *Pan*. (f) The lateral epicondyle is well developed. (g) The capitulum is large and is separated from the deeply notched trochlea by a marked groove. (h) The coronoid fossa is not deep and the radial fossa is poorly developed. The olecranon fossa is deep and the steep medial wall of the fossa forms part of the powerful buttressing to the medial epicondyle.

Significance of 1970 Collection

The 1970 collection of hominid material from East Rudolf, combined with the specimens collected during the 1968 and 1969 expeditions, adds up to an appreciable sample of early Pleistocene hominids from a single, dated locality. The material is complementary to other collections from various East African localities in some respects and the evaluation of the enlarged sample should assist study of man's origins and evolution during the Plio/Pleistocene times.

Most of the specimens from East Rudolf—a total of nineteen individuals—can be assigned to the genus *Australopithecus*. For the present, these individuals are not being assigned to species. The evidence indicates that a population of australopithecines lived in the region for at least one million years, the individuals which have been recovered being random examples of this population. In such circumstances, it is reasonable to expect a fairly wide range of individual variation. Sexual and allometric differences must also be taken into account when species identification is finally considered.

The gracile demi-cranium KNM-ER-732 was located at the same site but from a slightly higher horizon from which the large australopithecine cranium was collected in 1969. The material has yet to be studied in detail but it seems likely that the two specimens represent the two sexes of the same species. Sexual dimorphism is known to be pronounced in the large primates; the most impressive exception to this is *Homo sapiens* which has, however, developed such a complex behavioural pattern as to be incomparable with the other primates in this context.

The two femora KNM-ER-736 and 738 have been tentatively assigned to the genus *Australopithecus*. The principal difference between the two specimens is size and, although the two finds are from two different stratigraphic units, it is possible that they may represent respectively male and female individuals of same species of australopithecine. The mandibular and cranial remains indicate that the species changed little between the Koobi Fora IIA, IIB and III units.

It might be argued that the large australopithecines at East Rudolf represent the robust form now known from many sites in Africa and the small, gracile specimens are the East Rudolf equivalent of the gracile australopithecine best known from the South African cave sites. The two species hypothesis at Rudolf seems unlikely, however, and its presentation would be unwise in the light of the present evidence.

The occurrence of *Homo* at East Rudolf is suggested by the three specimens KNM-ER-730, 731 and 737. These are from stratigraphic levels that have yielded specimens of both sizes of australopithecine at East Rudolf and that they were contemporaneous is not in doubt. The relationship of the East Rudolf *Homo* material to other occurrences of Lower Pleistocene *Homo* in East Africa is a matter for further study.

There seems no evidence, however, that the genus *Homo* at Rudolf had any direct relationship to the australopithecine population of the same time and with which it shared its habitat. The concept of the gracile australopithecine being ancestral to *Homo* in the Lower Pleistocene requires careful re-examination on the basis that at Rudolf, the gracile variety may prove to be the female of the robust form. The robust australopithecine is now widely accepted as a highly specialized side branch which made little or no evolutionary advance during the Pleistocene. The specimens from East Rudolf support this view and there is no evidence of significant changes occurring between the early specimens in the lower part of the sequence and the later ones at the top of the section.

The interesting postcranial remains will be studied in detail, with particular emphasis on the functional aspects of the various bones. The specimens seem to represent two distinct forms: the one femoral shaft KNM-ER-737 is very close to material attributable to *Homo* and is suggestive of an upright,

bipedal hominid; the rest of the material is distinctive and presumably should be assigned to *Australopithecus*. This australopithecine material suggests a form of locomotion that was not entirely upright nor bipedal. The Rudolf australopithecines, in fact, may have been close to the "knuckle-walker" condition, not unlike the extant African apes. Further study will reveal the true position and, of course, there is no reason to assume that the locomotion of the australopithecines has any living model. The post-cranial material, like the cranial material, suggests therefore that there were two distinct hominids at Rudolf in the Lower Pleistocene.

In spite of the lack of species identification here, the general conformity between the robust australopithecines of Rudolf, the Omo and Olduvai should be noted. Unfortunately, the material from Olduvai is limited to two specimens, and from Omo there are several mandibles, an immature cranial fragment and numerous teeth. Continued research may provide adequate grounds for maintaining an East African species of australopithecine (*A. boisei*) and a distinctive southern species (*A. robustus*). Australopithecine taxonomy and evolution are however, as complex as those of early *Homo*, but the Rudolf material seems to confirm the view developed as a result of

work at Olduvai and the Omo that *Homo* and *Australopithecus* are two quite separate and distinct early Pleistocene hominids.

I thank the government of the Republic of Kenya for permitting the research at Rudolf to be undertaken; the trustees of the National Museum for allowing me to lead the research at Rudolf; the participants of the 1970 expedition and, in particular, Mr Kamoya Kimeu, who assisted with the planning and execution of the field season; and the National Geographic Society, the William H. Donner Foundation, the L. S. B. Leakey Foundation and several individual donors for financial support.

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Preliminary Stratigraphical Studies of the East Rudolf Basin, Kenya

CARL F. VONDRA, GARY D. JOHNSON & B. E. BOWEN

Department of Earth Science, Iowa State University, Ames, Iowa 50010

A. K. BEHRENSMEYER

Department of Geology, Harvard University, Cambridge, Massachusetts 02138

The investigation of the Plio-Pleistocene sediments in the East Rudolf area was continued in the summer of 1970. This article places the important hominid sites in their geological context.

BEFORE 1968, the primary source of palaeontological and geological information about the area to the east of Lake Rudolf was the report of Teleki and Von Hohnel's expedition in 1888. Von Hohnel collected invertebrate fossils and rock specimens and reported the occurrence of beach ridges and lavas near the lake¹. Fuchs traversed the area in 1935 but did not comment extensively on the geology². Preliminary investigations of the Plio-Pleistocene sediments in the East Rudolf Basin by a National Museum of Kenya expedition in 1968 and 1969 have shown the area to be of considerable significance to the palaeontological, palaeoanthropological and geological history of East Africa³⁻⁷. The geological investigation of the area initiated by A. K. B. in 1969⁴ was continued in the summer of 1970 as part of the National Museum of Kenya expedition. This article outlines the general stratigraphy and depositional environments of the sedimentary units studied in the area.

The area lies between latitude 3° 45' and 4° 20' N. and longitude 36° 10' and 36° 25' E. It is largely a semi-arid region of low relief locally dissected by ephemeral streams so that the topography is quite rugged. The East Rudolf Basin is located near the northern end of the Gregory Rift Valley which cuts across the southern end of the lake and trends in a north-

north-easterly direction. The area studied lies on the north-western flanks of this trend (Fig. 1).

The East Rudolf sedimentary terrain can be separated into two smaller areas separated by the Kokoi horst-Suregei plateau structure. Each one will be dealt with separately in view of the present difficulty in stratigraphic correlation between these two areas.

Ileret Area

The northern portion of the East Rudolf sedimentary terrain, here known as the Ileret area, consists of four unconformable sedimentary units limited on the east and south by recently faulted and uplifted Mio-Pliocene volcanics. The lowest unit comprises 45-65 m of fossiliferous and predominantly fluvial sediments. The lowest 50 m, yielding all of the hominid specimens collected so far (Fig. 2), is characterized by interbedded channel, levee, and floodbasin mudstones and subarkoses intercalated with two tuffs which serve as local stratigraphic markers. A W.N.W. to N. palaeocurrent component predominates in the channel sandstones with pedogenic modification (both hydromorphic and calcic) occurring on most proximal floodplain deposits. Conformably overlying this lower fluvial sequence is a 5 m lacustrine deposit of bentonitic mudstones and subarkoses containing a Nilotic fish fauna (*Lates niloticus*). This in turn is followed conformably by a second fluvial sequence of channel, levee, and floodbasin sediments having a character very similar to the lower fluvial sequence. Palaeocurrent trends again reflect a W.N.W. component. Capping the entire lowest unit at Ileret is a 2-3 m tuff containing well rounded pumice pebbles and cobbles and local zones of pedogenic calcic horizons.

Following deposition of the lowest unit, the entire sequence was subjected to regional compression resulting in N.E.-S.W. trending flexures and faults. An erosional unconformity following this phase resulted in some 10 m of relief on the underlying sediments.

A fluvial channel complex with S.S.E. directional components developed on this unconformity is the first stage in the sedimentation of the middle unit as exposed in the Ileret region. Calcic palaeosols, crotonina, and calcic root concretions are common in this 7 m sequence. Overlying the basal portion of this unit is a 20 m sequence of shallow water lacustrine mudstones and subarkoses interbedded with one to five tuffs and containing a complex Nilotic fauna of molluscs, crocodile, and fish (*Lates*, *Polypterus* (?)). Algal stromatolites and gastropod-algal mat sediments evidencing shallow water desiccation ponds are abundant. A 7 m fluvial-deltaic complex completes the sedimentary record of the middle unit and the lithology is similar to the sediments of the basal portion. The paucity of terrestrial fauna in this member has so far precluded the recovery of any hominid remains. Sedimentation culminated in an episode of tuff deposition which was followed by a renewed period of tectonism.

An erosional unconformity in conjunction with E.-W. trending compressional forces (creating N.-S. trending flexures, normal faults and small scale horst and graben features) resulted in a truncation of the middle Ileret unit by some 10 m.

The upper unit of the Ileret sequence, here informally named the Galana Boi (Gabbra for Lake Rudolf) unit, represents two separate stages in the late Pleistocene stand of Lake Rudolf. This lacustrine and fluvial-deltaic sequence exhibits high lateral variability. Algal stromatolites and *Etheria* banks are

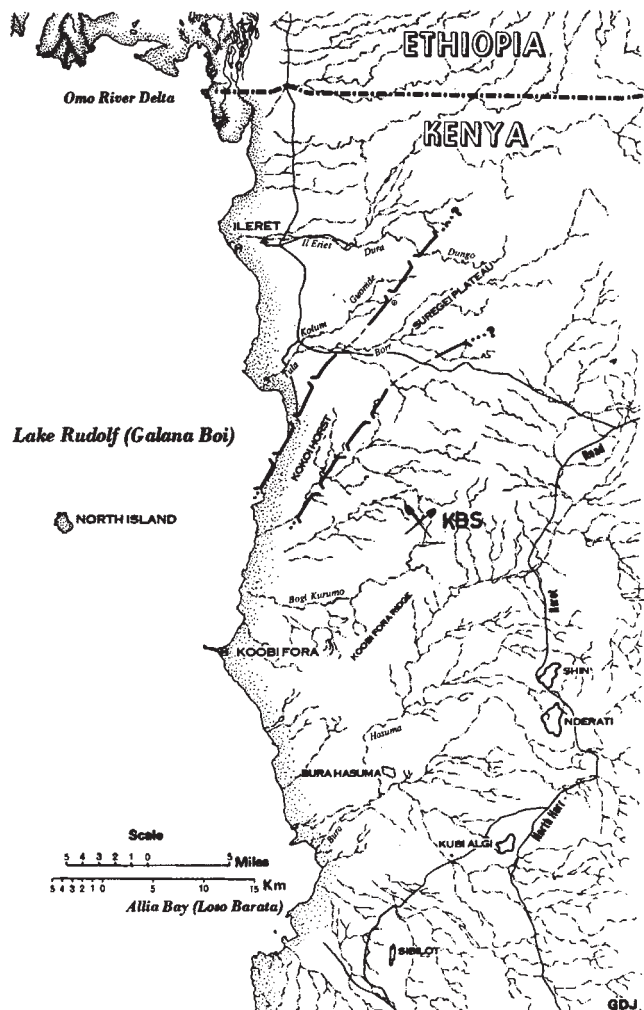


Fig. 1 Outline map of the principal geographic features of the East Rudolf Basin, Kenya.

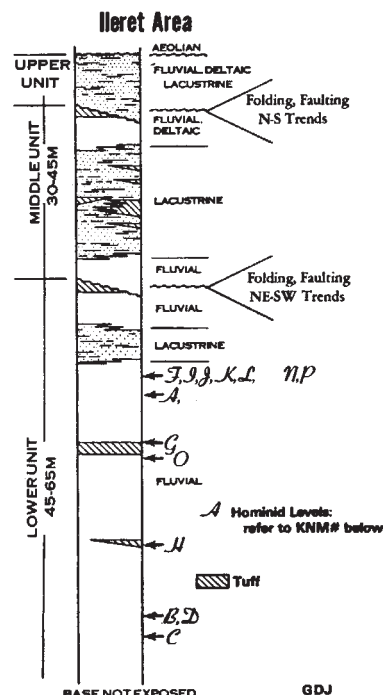


Fig. 2 Generalized stratigraphic section of the Ileret sedimentary sequence. Important hominid-bearing horizons are indicated. A, KNM-ER-404; B, KNM-ER-406; C, KNM-ER-405; D, KNM-ER-732; E, KNM-ER-726; G, KNM-ER-729; H, KNM-ER-727; I, KNM-ER-731; J, KNM-ER-739; K, KNM-ER-740; L, KNM-ER-728; N, KNM-ER-741; O, KNM-ER-733; P, KNM-ER-725.

common in the lacustrine facies. A regional unconformity followed by the deposition of aeolian gravelly subarkoses caps the entire sequence.

The pre-sedimentary terrain of the Ileret area consists of recently faulted and uplifted composite flows of fine-grained olivine basalt presently dated at 11.6 ± 0.5 m.y. BP for the Suregei plateau sequence to the east, and a probable similar age for the Kokoi horst sequence to the south. (Since the time of writing, the Kokoi Volcanic sequence has been dated at 3.62 ± 0.36 m.y. BP for one of the youngest flows.) The significance of these flows lies in the fact that there are interbedded lacustrine mudstones and tuffs with a sparse Nilotic fauna in the case of the Kokoi beds and a rather complex sequence of deeply weathered (profile depth: 7-10 m) pale-oxisols and locally derived torrential fluvial deposits within the Suregei beds. This Mio-Pliocene sedimentary and volcanic complex may yield critical faunal remains which will be valuable in the study of mammalian evolution in this area. The analytical details of three conventional whole rock degassing K-Ar age determinations on a fine-grained flow aligned olivine basalt from the top of the Suregei plateau sequence are given in Table 1. (Age determinations were carried out by F. J. Fitch (Department of Geology, Birkbeck College, London) and J. A. Miller (Department of Geology and Geophysics, University of Cambridge).)

Recent uplift of this basement complex is apparent from the variable composition of high and low level river terraces along the Il Eriet. Here a high level (30 m) terrace is veneered with predominantly acid igneous lag, while a lower (7-10 m) terrace contains mostly basic volcanics. Folding and truncation of the sediments of the lower, middle and upper units of the Ileret sequence adjacent to the Kokoi horst further substantiate this uplift.

Koobi Fora-Allia Bay Area

An embayment of the ancestral Lake Rudolf was present in the Koobi Fora-Allia Bay region during much of the period represented by the Plio-Pleistocene sediments. Five stratigraphic units based on characteristics of lithology have been differen-

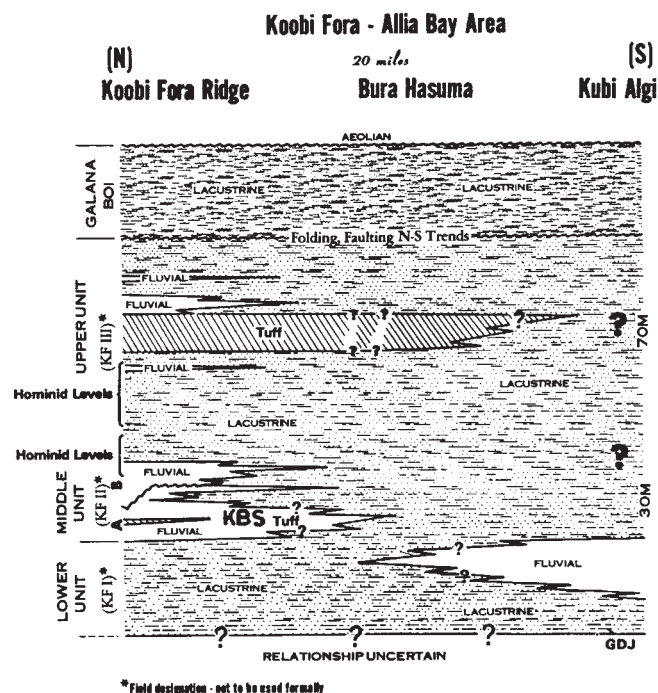


Fig. 3 Schematic facies diagram of the Koobi Fora–Allia Bay segment of the East Rudolf sedimentary basin. The important hominid levels are indicated.

tiated. Their lateral relationships are complex and will not be discussed in detail here. Description of the stratigraphic succession is a simplified interpretation which is relevant to the sequence of fossil and artefact-bearing horizons (Fig. 3).

The lowermost unit, referred to as Koobi Fora I (KF-I)⁴ (the nomenclature Koobi Fora I, II, and so on is being retained as an informal field designation for units which will eventually be given formational names), consists of a series of deep water to littoral lacustrine claystones, siltstones and sandstones. The finer-grained sediments occur in bodies up to 10 m thick and are often thinly bedded, finely laminated and rhythmically intercalated. Limonite nodules of varying sizes occur throughout. The clay and siltstones are gypsiferous, with crystals and veins of selenite and satinspar occasionally abundant. Structureless to planar cross-stratified, sheet-like bodies of oligomictic conglomerates and subarkoses are interbedded. A particularly distinctive continuous sheet of conglomerate occurs in the upper part of the unit north-east of the Koobi Fora peninsula. It consists of imbricated algal-encrusted disks and blades of locally derived limonite clasts up to 15 cm in diameter. The matrix is a cross-stratified to structureless subarkose. Pelecypod burrows are common and often penetrate the underlying bed.

In the south near Kubi Algi the lower part of this lacustrine sequence interfingers with about 20 m of fluvio-deltaic sediments. These consist of channel sandstones and associated overbank-type siltstones and mudstones with at least six thin, discontinuous tuff lenses. The fluvial tongue seems to represent a deltaic plain of a local stream system entering the lake

embayment from the south-east. The deposits are fossiliferous, containing banks of *Etheria* and pockets of mammalian remains. The fauna⁸ indicates an age slightly older than Kanapoi and Lothagam-III. K-Ar determinations by Fitch and Miller on sanadine crystals in pumice fragments from a tuff conformably underlying the fossiliferous beds (about 5 m below) indicate an age of 4.55 ± 0.1 m.y. BP, lending support to the faunal interpretation.

In the north-eastern and central parts of the area, the Lower Unit (KF-I) is conformably overlain by a tongue of fluvial sediments which include conglomerates, subarkoses, siltstones, mudstones, palaeosols and an important tuff horizon. This unit has been referred to as Koobi Fora II (KF-II) and is divided into two subunits, A and B, by an unconformity displaying at least 10 m of relief⁵. The lower subunit (KF-IIA) is characterized by lenticular and sheet-like bodies of trough cross-stratified clay gall conglomerates and poorly sorted subarkoses, sheets of sandy siltstones, mudstones and associated calcic and hydromorphic palaeosols. A discontinuous artefact bearing tuff, the "KBS Tuff", occurs about 5 m above the base of the unit in the north-eastern part of the area. This tuff was preserved in a variety of environments ranging from floodplain (overbank and moist floodbasin swale situations) to point bars and channel-fills developed in a stream system with a general S.W. palaeocurrent direction. Abraded but otherwise fresh pumice from the tuff has been dated at 2.61 ± 0.26 m.y.⁵.

Overlying the unconformity are 25–30 m of fluvial polymictic conglomerates, subarkoses and siltstones deposited on a deltaic plain extending south-westward into the lake embayment. These deposits form the upper subunit, KF-IIB, which grades laterally into lacustrine sediments in the vicinity of the Koobi Fora peninsula.

The fluvial sediments of KF-II grade upward into a 70 m thick sequence of shallow lacustrine and deltaic deposits which are preserved only within about 5 miles from the present lake shore, where they have been downfaulted along a north–south tectonic trend. This Upper Unit has been given the temporary designation Koobi Fora III (KF-III). The lithology is very variable, with fine-grained ripple-laminated and planar cross-stratified subarkoses and siltstones, thin beds of shallow lacustrine algal beds and stromatolites, and littoral, molluscan sandstones. Occasional fluvial bodies and lenses of coarse-grained subarkose and polymictic conglomerates interfinger with the lacustrine and deltaic sediments. Vertebrate remains are abundant throughout most of the unit. The entire sequence grades rapidly to the east and south into a sequence of deep lacustrine claystones and siltstones. A prominent tuff up to 12 m thick occurs in the middle of KF-III and can be traced into the deep-water lacustrine facies. The tuff is locally trough-stratified and ripple-laminated and often contains high admixtures of quartz sand, silt and shell fragments as well as pumice cobbles⁶.

Isolated outliers of a once extensive lacustrine unit overlie the older sediments on a pronounced, sometimes angular, unconformity. This unit is considered to be partially equivalent to the Galana Boi beds at Ileret. (At Ileret, the lower level, but stratigraphically youngest, Galana Boi beds have been recently dated at 9360 ± 135 BP.) It is a maximum of 30 m thick and the typical lithology is a grey, tuffaceous siltstone to claystone with varve-like laminations and bands of limonitic nodules. Molluscs are abundant, occurring in distinct packed coquinas or dispersed in the siltstones. The sediments are vertically homogeneous with the exception of about a metre of conglomeritic subarkose at the base. A mantle of Holocene gravels and aeolian gravelly subarkoses overlies the Plio-Pleistocene section and seems to be derived in part from the Galana Boi and older deposits.

A period of regional compression, producing a broad north–south anticline, followed by a period of tensional stresses which caused faulting and the formation of horst and graben structures, affected the sediments just east of the Koobi Fora peninsula. Tectonic activity occurred between the deposition of

Table 1 Results of Conventional Whole Rock Total Degassing K-Ar Age Determination on the Youngest Suregei Basalt

Sample	Method	K ₂ O atmospheric contamination (%)	v/m	Apparent age and error (m.y.)
ER-70-20-0112 (FM7040)	K-Ar	1.41 39.1	5.56×10^{-4}	11.8 ± 0.5
	40-50	1.41 43.6	5.36×10^{-4}	11.4 ± 0.5
	TR	1.41 38.7	5.45×10^{-4}	11.6 ± 0.5
	Average apparent age = 11.6 ± 0.5 m.y.			

v/m = volume of radiogenic argon-40 (mm³ NTP) per g of sample; $\lambda\beta = 4.2 \times 10^{-10}$ yr⁻¹; $\lambda\epsilon = 0.584 \times 10^{-10}$ yr⁻¹.

KF-III and the Galana Boi deposits. Normal faulting has also affected sediments east of this zone, but no regional trends have been observed.

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³ Leakey, R. E. F., *Nature*, **226**, 223 (1970).

⁴ Behrensmeyer, A. K., *Nature*, **226**, 225 (1970).

⁵ Fitch, F. J., and Miller, J. A., *Nature*, **226**, 226 (1970).

⁶ Leakey, M. D., *Nature*, **226**, 228 (1970).

⁷ Dixey, F. I., *Report of the UMC/UNESCO Seminar on the East African Rift System* (University College, Nairobi, 1965).

⁸ Maglio, V., *Nature*, **231**, 248 (1971).

Vertebrate Faunas from the Kubi Algi, Koobi Fora and Ileret Areas, East Rudolf, Kenya

VINCENT J. MAGLIO

Museum of Comparative Zoology, Harvard University, Cambridge, Massachusetts 02138

A rich collection of vertebrates has been recovered from the three principal fossiliferous localities at East Rudolf. Two faunal units, one of Pliocene and the other of Pleistocene age, are represented. Two of the elephant species recovered represent the earliest known occurrences of the *Loxodonta* and *Elephas* phyletic lineages.

MORE than 600 specimens of vertebrate fossils have now been collected from the three major fossiliferous areas at East Rudolf—Kubi Algi, Koobi Fora and Ileret. Preliminary identifications have been made on some of the material including the Primates (M. and R. Leakey), Suidae (H. B. S. Cooke), Bovidae (J. Harris) and Proboscidea (V. Maglio), but more detailed studies must await further collections and geological analyses of the fossil bearing horizons.

Two faunal units are represented. One is poorly known at present but certainly Pliocene in age; the other is early Pleistocene. The younger unit covers a fair time range within which definite evolutionary changes are apparent in several groups, but no distinct division of the unit is possible on faunal grounds. This faunal unit extends across the regional disconformity in the Koobi Fora area.

Koobi Fora I

The earlier fauna is from a fluvial tongue within Koobi Fora I¹ in the Allia Bay area, near the mountain Kubi Algi. Fossil material derives from two sites that seem to have been contemporaneous on both palaeontological and geological evidence. Two elephant species represent the earliest known occurrences of the *Loxodonta* and *Elephas* phyletic lineages. A fragmentary skull and a nearly complete mandible of *Loxodonta adaurora* are smaller and somewhat more primitive than the Kanapoi–Mursi Formation stage, especially in the upper dentition. This would be expected from the earlier age of the KF-I deposits (discussed later). *Elephas ekorensis* is represented by a partial skull with complete upper third molars. It is less evolved than the Kanapoi–Ekora form, although it is similar in cranial morphology. A small fragment of a *Gomphotherium* molar adds further evidence for the antiquity of the fauna. This genus does not occur in East Africa later than the late Pliocene Kanapoi horizon.

The suids include a *Nyanzachoerus* mandible, too fragmentary for specific identification, and two skulls of *Notochoerus capensis* with nearly complete dentitions (personal communication from H. B. S. Cooke). Other species in the

Kubi Algi fauna include *Deinotherium* sp., *D. cf. bozasi*, *Hippopotamus* sp., *H. cf. immagunculus*, and a bovid skull with affinities to the Asiatic genus *Leptobos*.

Koobi Fora II and III

The large vertebrate assemblage from KF-II and KF-III along the Koobi Fora ridge includes a variety of Primates provisionally identified as *Papio* sp., *Simopithecus* sp., *Colobus* sp., *Cercopithecus* sp., *A. cf. boisei*², and *Homo* sp.³. *Simopithecus* is the best represented species, known from cranial and post-cranial remains as well as from dentitions.

Several species of carnivores include *Crocota* sp., *Hyaena* sp. and a canid close to the living jackal, *Canis mesomelas*. Two felids, one very large, are also represented, but are too poorly known at present for positive identification. A small machairodont is known from excellent cranial and mandibular material.

The dominant proboscidean is *Elephas recki*, well known from several skulls and jaws in addition to numerous isolated molars. An earlier form, Stage Two⁴, is represented in KF-II and is the typical form in the Shungura Formation of the Omo basin. The more progressive Stage Three seems to be present in KF-III, but additional material is needed for confirmation. *Loxodonta adaurora* is known only from several teeth in KF-II and its presence here confirms the persistence of this species into the early Pleistocene. *Deinotherium* sp., *D. cf. bozasi* is a rare element in the KF-II fauna and has not yet been recorded from KF-III.

Equus occurs throughout the KF-II and KF-III section and is known from isolated teeth and several mandibular rami. *Stylohipparion albertense* is rare. An early form of the white rhinoceros, *Ceratotherium simum germanoaffricanum*, is present along with the black rhinoceros, *Diceros bicornis*. The latter has proportionately longer molars and larger premolars than in the living representative of that species and may eventually prove to be different at the subspecies level.

At least three suid species are common in KF-II and KF-III, *Notochoerus scotti*, *Metridiochoerus andrewsi* and *Mesochorus limnetes*. The latter two species are known from excellent cranial material. The KF-II specimens of *M. limnetes* have low-crowned molars with short talons on M3 and relatively little cement. They closely resemble material from the Shungura Formation between Tuffs D and H. The KF-III material is higher crowned with more complex talon development and has a greater cement cover. In these regards this stage is like specimens from the Shungura Formation above Tuff H and approaches the condition found in *Mesochorus olduvaiensis* from Olduvai Gorge. Near the top of the sequence in KF-III an *Afrochoerus* resembling, but more primitive than, *A. nicoli* from Kanjera first makes its appearance. Although similar to *A. nicoli* in its molar teeth, the Koobi Fora material is less progressive in the structure and implantation of the canines.

Table 1 Provisional List of the Vertebrate Species and their Distribution in the Koobi Fora Stratigraphic Sequence

	I	II	III
Primates			
Cercopithecidae			
<i>Cercopithecus</i> sp.		x	x
<i>Colobus</i> sp.		x	x
<i>Papio</i> sp.		x	
<i>Simopithecus</i> sp.	x	x	x
Hominidae			
<i>Australopithecus cf boisei</i>		x	x
<i>Homo</i> sp.			x
Rodentia			
<i>Hystrix</i> sp.		x	x
<i>Thyromys</i> sp.			x
Carnivora			
Hyaenidae			
<i>Crocuta</i> sp.		x	x
<i>Hyaena</i> sp.		x	x
Canidae			
<i>Canis</i> sp., <i>C. cf mesomelas</i>		x	
Felidae			
<i>Panthera</i> sp.		x	x
<i>Acinonyx</i> sp.			x
Machairodontinae			
gen. et sp. indet.		x	
Mustelidae			
<i>Lutra</i> sp.		x	
Proboscidea			
Gomphotheriidae			
<i>Gomphotherium</i> sp.	x		
Elephantidae			
<i>Elephas ekorensis</i>	x		
<i>Elephas recki</i>		x	x
<i>Loxodonta adaurora</i>	x	x	
Deinotheroidea			
<i>Deinotherium</i> sp., <i>D. cf bozasi</i>	x	x	
Perissodactyla			
Equidae			
<i>Equus</i> sp.		x	x
<i>Stylohipparion albertense</i>		x	x
Rhinocerotidae			
<i>Ceratotherium simum germanoaffricanum</i>		x	x
<i>Diceros bicornis</i> sp.		x	x
Artiodactyla			
Hippopotamidae			
<i>Hippopotamus</i> sp., <i>H. cf immagungulus</i>	x	x	x
<i>Hippopotamus protamphibius</i>		x	
<i>Hippopotamus protamphibius</i> (progressive)			x
Suidae			
<i>Nyanzachoerus</i> sp.	x		
<i>Notochoerus capensis</i>	x		
<i>Notochoerus scotti</i>		x	x
<i>Metridiochoerus andrewsi</i>		x	x
<i>Mesochoerus limnetes</i> (typical)		x	
<i>Mesochoerus limnetes</i> (progressive)			x
<i>Afrochoerus</i> sp.			x
<i>Phacochoerus</i> sp., <i>P. cf aethiopicus</i>			x
Giraffidae			
<i>Giraffa gracilis</i>		x	x
<i>Giraffa</i> sp., <i>G. cf jumae</i>		x	x
<i>Libytherium</i> sp.		x	x
Bovidae			
<i>Connachaetes</i> sp. nov.		x	
<i>Damaliscus</i> sp.		x	x
<i>Gazella</i> sp. <i>G. cf wellsi</i>			x
<i>Hipotragus gigas</i>			x
<i>Kobus sigmoidalensis</i>		x	x
<i>Menelikia</i> sp. nov.		x	x
<i>Oryx</i> sp.		x	
<i>Pelorovis</i> sp.		x	x
<i>Redunca ancystrocera</i>			x
<i>Strepsiceros</i> sp., <i>S. cf grandis</i>		x	x
<i>Tragelaphus nakuae</i>			x
Aves			
gen. et sp. indet.		x	x
Reptilia			
<i>Crocodylus niloticus</i>		x	x
<i>Euthecodon brumpti</i>		x	x
<i>Geochelone</i> sp.		x	x
Squamata, gen. et sp. indet.		x	x
Osteichthyes			
<i>Clarius</i> sp.		x	x
<i>Lates</i> sp.		x	x
<i>Polypterus</i> sp.		x	x

It may have been broadly ancestral to later species. A single mandible of *Phacochoerus* sp., *P. cf aethiopicus* was found on the surface near the top of the section, but this may have been derived from the late Pleistocene Galana Boi beds that overlie the earlier beds in many areas (personal communication from H. B. S. Cooke).

The small *Hippopotamus* sp., *H. cf immagunculus*, carries through from KF-I and persists into KF-III without significant morphological change. A larger species identical with *H. protamphibius* is known from several skulls and jaws from KF-II and is succeeded in KF-III by a still larger and more progressive stage.

Three distinct giraffids are present in the KF-II and KF-III faunas. A small species is referable to *Giraffa gracilis* and is represented by several mandibles, a partial skull and isolated teeth. A larger form, known from a partial cranium, a "horn" and a mandible, differs from the living *G. camelopardalis* in a number of features and is closer to the Olduvai species, *Giraffa jumae*. A large *Libytherium* is known from isolated teeth and a nearly perfect skull. It is clearly not referable to *L. olduvaiensis*, but its relationships are not yet determined.

Rodents are poorly represented at present except for a large *Hystrix* of which several mandibles and a palate have been collected. A second species, referred to the genus *Thyromys* is known from KF-III.

The Bovidae are the most numerous and varied elements of the fauna with at least twelve species represented (personal communication from J. Harris). The following species have been identified provisionally from KF-II and KF-III: *Kobus sigmoidalensis*; *Strepsiceros* sp., *S. cf grandis*; *Damaliscus* sp.; *Pelorovis* sp; and *Menelikia* sp. nov. A new species of *Connachaetes* is known so far only from KF-II, as is *Oryx*. *Redunca ancystrocera*, *Hippotragus gigas*, *Gazella* sp., *G. cf wellsi* and *Tragelaphus nakuae* are recorded only from KF-III. Because of the relatively few specimens involved, it is possible that the distinction between KF-II and KF-III will disappear with further collecting.

The fish *Clarias* sp. and *Lates* sp. are well represented and several specimens of *Polypterus* sp. have been collected. Two crocodilians, *Crocodylus niloticus* and *Euthecodon brumpti*, are known from several complete specimens. A large tortoise, *Geochelone*, occurs up to two metres in diameter. Several lizards are known from vertebrae and a partial skull.

Some bird remains, chiefly limb bones, have been recovered from both KF-II and KF-III.

Ileret Lower Unit

An abundant fauna has been collected from the area around Ileret. The succession as seen in the Suidae and Hippopotamidae as well as other faunal evidence would suggest that the Ileret Lower Unit¹ is roughly contemporaneous with the entire KF-II and KF-III sequence farther to the south. The appearance of *Afrochoerus*, a large form of *Hippopotamus protamphibius* and an advanced form of *Mesochoerus limnetes* toward the top of the Lower Unit at Ileret is paralleled at Koobi Fora the appearance of the same elements towards the top of KF-III. On faunal grounds the tuff marking the top of the Ileret Lower Unit seems temporally equivalent to the KF-III tuff.

Radiometric age determinations for the Koobi Fora, Ileret and Kubi Algi tuffs will provide a good basis on which correlations can be made with other East African sites. At present, the faunal evidence from Koobi Fora II suggests a close parity with the fauna from the Shungura formation above Tuff D. The Koobi Fora III material seems closest to the fauna known from the uppermost part of the Shungura formation and also Lower Olduvai, that is Bed I and lower Bed II.

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² Leakey, R. E. F., *Nature*, **226**, 223 (1970).

³ Leakey, R. E. F., *Nature*, **231**, 241 (1971).

⁴ Maglio, V. J., *Nature*, **225**, 328 (1970).

Geographical Variation in the Atlantic Salmon

R. H. PAYNE, A. R. CHILD & A. FORREST

Ministry of Agriculture, Fisheries and Food, Salmon and Freshwater Fisheries Laboratory, Weymouth, Dorset

The discovery of a serum protein polymorphism in the Atlantic salmon has revealed the existence of genetically isolated populations sufficiently distinct to merit their separation into subspecies and races.

THE Atlantic salmon is an interesting organism for studies on the genetics of raciation because individuals spend some part of their lives at sea, often at considerable distances from the rivers in which they were spawned, yet they tend to return to their maternal rivers to breed. Although salmon mix freely in the sea, this segregation before breeding creates a situation where the population of every river exhibits a degree of genetic isolation from populations in adjacent river systems. It is therefore very likely that genetic heterogeneity exists between salmon populations in different river systems and that polymorphisms are restricted closely to the region in which the original mutant allele appeared. This being the case, it should be possible to delimit races of Atlantic salmon by discovering sharp discontinuities in the free spread of mutant alleles through the geographical range of the species.

The discovery that salmon from rivers on both sides of the Atlantic Ocean migrate to communal feeding grounds off Greenland¹⁻⁵, and the subsequent establishment of a high-seas drift net fishery in the Davis Strait between Greenland and Canada, has necessitated the development of methods to assess the contribution of various North Atlantic national salmon stocks to this fishery.

One approach to this problem is to establish, from a study of genetically determined serum protein polymorphisms, whether raciation has taken place in the Atlantic salmon; and if so, to delimit the geographical boundaries of these races. Clearly, in the case of anadromous fishes, the geographical boundaries of races must be considered in terms of breeding areas rather than the maritime dispersal of smolts.

The analysis of genetic variation in serum proteins by electrophoresis in starch gels⁶ offers a convenient method for detecting discontinuities in free gene exchange within species, thereby delimiting genetically distinct "stocks" or races. This approach to microsystematics has been of particular value in the study of raciation and "stock identification" in commercially important marine fishes such as cod⁷⁻⁹, tuna¹⁰ and herring¹¹.

Transferrin Polymorphism

Transferrin (siderophilin) is a beta-globulin found universally in vertebrate serum. This protein has a high binding capacity for iron and it is likely that its biological role is the distribution of this element in the circulatory system^{12,13}.

Genetic polymorphism at the transferrin locus has been described for many vertebrate species and the investigation of transferrin polymorphism is now a well established approach to problems in microsystematics.

This article presents the results and conclusions of an analysis by starch gel electrophoresis of the serum transferrin phenotypes of 4,492 individuals collected during the summers of 1969 and 1970 at several locations in Britain, Ireland and the Gulf of Saint Lawrence in Canada.

Three transferrin phenotypes, Tf1, Tf1/Tf2 and Tf2, have been observed in populations of Atlantic salmon sampled in Britain and Ireland. Electropherograms of these phenotypes are illustrated in Fig. 1. The distribution of phenotype classes in the British Isles is presented in Table 1.

Table 1 Transferrin Phenotypes of British and Irish Populations of the Atlantic Salmon

Sampling station	No. of fish sampled of each phenotype			
	Tf1	Tf1/Tf2	Tf2	Total
R. Teign, S. Devon	466	66	2	534
R. Severn	373	27	—	400
North Wales	194	10	—	204
R. Lune, Lancashire	377	7	—	384
Amble, Northumberland	529	3	—	532
R. Esk (N.), Angus	1,150	2	—	1,152
R. Bandon, Cork	147	5	—	152
R. Shannon	280	1	—	281
R. Moy, Mayo	408	2	—	410
R. Erne, Donegal	121	1	—	122
R. Foyle, Londonderry	243	—	—	243
Total	4,288	124	2	4,414

In the Canadian samples the Tf1 transferrin phenotype is present, but neither Tf1/Tf2 nor Tf2 were observed. However, four other phenotypes were detected in the Canadian material: Tf1/Tf3, Tf1/Tf4, Tf3/Tf4 and Tf4 (Fig. 1). The distribution of transferrin phenotype classes in the sample of seventy-eight individuals from the Gulf of Saint Lawrence was Tf1, 33; Tf3/Tf4, 19; Tf1/Tf3, 6; Tf4, 17; and Tf1/Tf4, 3.

The most satisfactory explanation for the observed transferrin polymorphism is that the transferrin phenotype is determined by four co-dominant allelomorphous genes: *Tf1*, *Tf2*, *Tf3* and *Tf4*. In the case of the sample from the River Teign in South Devon where the rare Tf2 phenotype was recorded, the observed phenotype ratio of 466:66:2 agrees very closely with the 467:65:2 expected ratio of a Hardy-Weinberg-Castle equilibrium.

Evidence for Subspeciation

The distribution of phenotypes between samples of European and North American origin favours the interpretation that the *Tf2* allele is restricted to European salmon and the *Tf3* and *Tf4* alleles are exclusively North American. Phenotypes resulting from the *Tf3* and *Tf4* alleles have been noted frequently in material sampled from the Davis Straits (Payne *et*

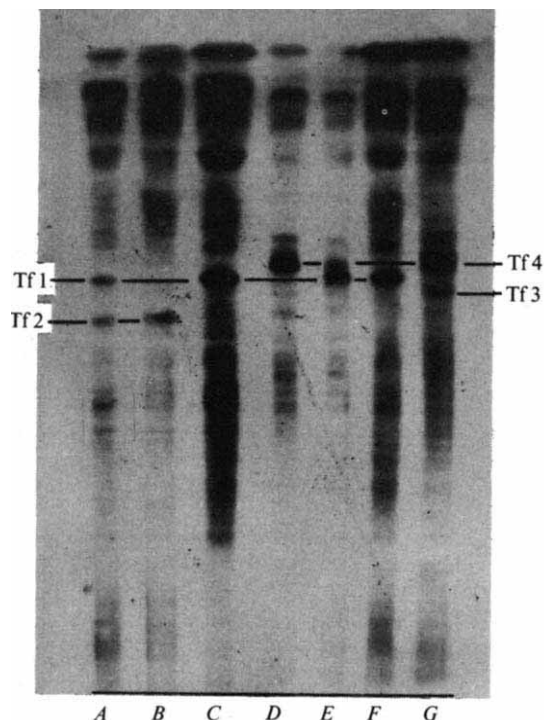


Fig. 1 Electropherogram in 12% starch gel of serum proteins of the Atlantic salmon in a discontinuous buffer system consisting of (1) gel buffer: 9.2% Tris adjusted with 1% citric acid to pH 7.6 and diluted ten-fold with water; and (2) vessel buffer: 1.85% boric acid adjusted to pH 8.6 with solid NaOH. Gels were stained for 15 min in a 0.1% solution of amido black 10B in 1:9 acetic acid-methanol and destained in several changes of 10% acetic acid. Four transferrin bands may be resolved with this system. Serum samples *A*, *B* and *C* were taken from fish in British rivers and demonstrate the transferrin phenotypes Tf1/Tf2, Tf2 and Tf1 respectively. Serum samples *D*, *E*, *F* and *G* were taken from salmon caught in the Gulf of Saint Lawrence, Canada, and demonstrate the transferrin phenotypes Tf4, Tf1/Tf4, Tf1 and Tf3/Tf4 respectively. Tf1 is the only phenotype common to populations in both continents.

al., unpublished results) and it would be very difficult to argue that these alleles are restricted to populations from the Gulf of Saint Lawrence. Gene exchange between Atlantic salmon from European and North American rivers is therefore negligible, or non-existent, except by human agency, and this isolation has been maintained for a period sufficient to permit genetic divergence. On this evidence therefore *Salmo salar* L. should be split into two subspecific taxa: *S. salar americanus* and *S. salar europaeus*.

Genetic theory indicates that Atlantic salmon may be classified by transferrin polymorphism into eight phenotype groups: Tf1, Tf1/Tf2, Tf2, Tf1/Tf3, Tf3, Tf3/Tf4, Tf4 and Tf1/Tf4. One phenotype, Tf1, is common to both subspecies but the Tf1/Tf2 and Tf2 phenotypes will be restricted exclusively to *S. salar europaeus* while the Tf1/Tf3, Tf3, Tf3/Tf4, Tf1/Tf4 and Tf4 phenotypes will be found only in *S. salar americanus*. Seven of the eight phenotypes predicted by theory have been observed in the samples.

As Tf1 is the only member of the allelomorphic group to occur throughout the range of the species and is the most common allele at this locus in both subspecies, it is suggested that this gene is ancestral and the other alleles have arisen by mutation since the establishment of the barrier to free gene exchange within the species.

Evidence for Racialization in the European Subspecies

The frequency of the Tf2 gene is highest in salmon populations from rivers in the southern part of the British Isles and

is lowest in the northern region; in the salmon population of the River Teign, South Devon, the estimated frequency of Tf2 is 0.065 while in the population of the River Esk (N.), which drains into the North Sea near Montrose, Angus, the frequency of this allele is reduced to 0.0009.

The River Teign population is significantly different with respect to transferrin phenotype ratios from the population of the River Severn ($0.01 > P > 0.001$) but there is no significant difference between the ratios of the phenotype classes in the samples from the River Severn, the River Bandon in south-east Ireland and a pooled sample of 204 fish from a number of rivers in North Wales ($0.1 > P > 0.05$). There is, however, a difference between the samples from North Wales and the River Lune in Lancashire but this is only just significant ($0.05 > P > 0.01$).

The four populations sampled from west and north-west Ireland (the estuaries of the River Shannon, Moy, Erne and Foyle) do not differ significantly from each other ($0.95 > P > 0.1$) but differ very significantly ($0.001 > P$) from the population of the River Bandon in south-east Ireland.

The interesting feature of these observations in the British Isles is not that transferrin polymorphism should exist (labiality at this locus is a common feature in many species of fishes), or that geographical variation in gene frequencies should be recorded (the reproductive habits of this species would tend to favour the establishment of such variation), but that such sharp discontinuities should be found (Fig. 2).

The high level of genetic isolation between the salmon populations of rivers draining into the North Sea and those of the English Channel can be readily explained by a consideration of the postglacial geomorphology of the British Isles. Before the Flandrian Transgression, when the sea level rose

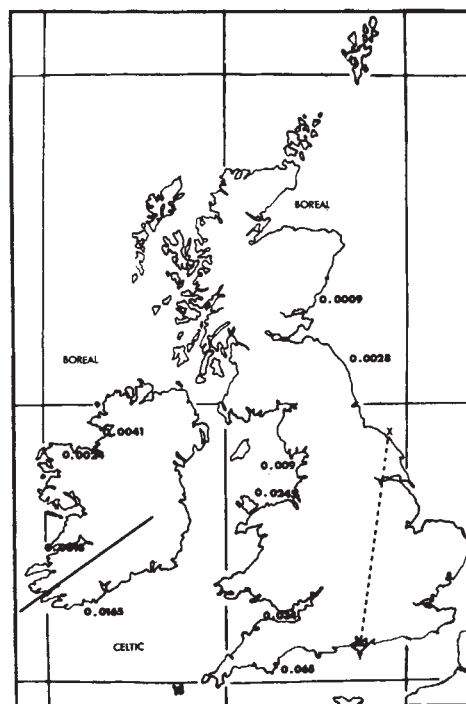


Fig. 2 Sketch map of the British Isles showing the geographical variation in the gene frequency of the Tf2 transferrin allele. The Atlantic salmon populations of the British Isles are sharply divided on the basis of the frequency of this gene into "Boreal" and "Celtic" races. A very sharp transition occurs in south-west Ireland where the gene frequency changes from 0.0165 to 0.0018 between the River Bandon and the River Shannon. At the present time no rivers draining into the North Sea and English Channel along the coastal region east of the approximate line X—Y have natural spawning populations of Atlantic salmon.

from perhaps -70 m to about its present level, Britain had been connected to the mainland by a strip of land some 300 miles wide. All the eastern rivers north of Kent drained directly into the North Sea or were tributaries of the River Rhine. The rivers of south-east England were then tributaries of the River Seine or drained directly into the English Channel.

There is no simple explanation, however, for the extremely low level of genetic introgression between the salmon of south-west Ireland and south-east Ireland, unless we postulate the existence of two distinct races, "Boreal" and "Celtic". These races do not exhibit any significant introgression and the sharp transition in the frequency of *Tf2* between the populations of the Rivers Bandon and Shannon indeed suggests the active maintenance of a barrier to such genetic introgression.

Graphical analysis of the data by plotting percentage cumulative frequencies on arithmetic probability paper (Fig. 3) clearly demonstrates the existence of the Boreal and Celtic races. The frequency of *Tf2*, however, is very significantly high in the Atlantic salmon population of the River Teign in South Devon. Clearly this population does not belong to the Celtic race and the evidence strongly suggests that it should be assigned to another race of more southerly distribution, probably extending across the English Channel into France.

Possible Mechanism for Racialization

The salmon population of the British Isles during the Last Interglacial Period would be forced progressively southwards as climatic conditions deteriorated. By the time of the maximum advance of the Würm I Ice Cap, conditions suitable for survival of Atlantic salmon would exist only in the extreme southern region of Britain and Ireland. As the ice cap receded in the Würm I-II Interstadial, recolonization northwards would take place, but with the advance of the Würm II Ice Cap the salmon population would again be forced to retreat (as an "interstadial relic fauna") to almost the original areas occupied at the height of the Würm I period of glaciation. Recolonization along the western coast of Ireland northwards into the Skagerrak and the estuary of the River Rhine would again have taken place in the Würm II-III Interstadial.

The final phase of the Last Glaciation which resulted in the formation of the Würm III Ice Caps over northern Britain and Scandinavia was much less severe than the two preceding glaciations. The Scandinavian salmon populations of the Würm II-III Interstadial would certainly have been eliminated

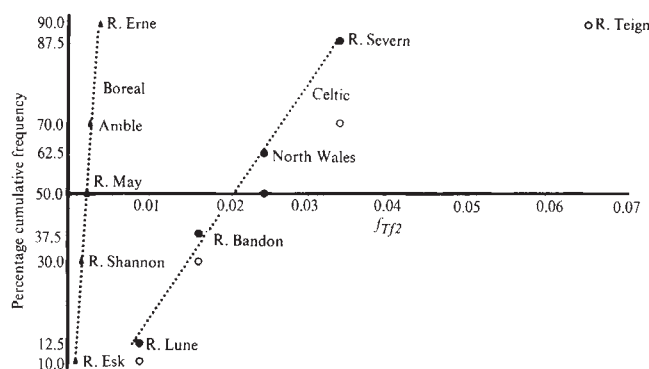


Fig. 3 Analysis of the geographical variation in the frequency of the *Tf2* transferrin gene by graphical display on arithmetic probability paper. The existence of the "Boreal" and "Celtic" races of Atlantic salmon around the British Isles is clearly demonstrated. The open circles indicate the distribution of the "Celtic" samples if the one from the River Teign is included; the position of that sample, however, is clearly anomalous and, if it is removed from the set, the distribution of the remaining four presents a much-improved straight line fit. The River Teign population possibly represents another race of European salmon of more southerly distribution.

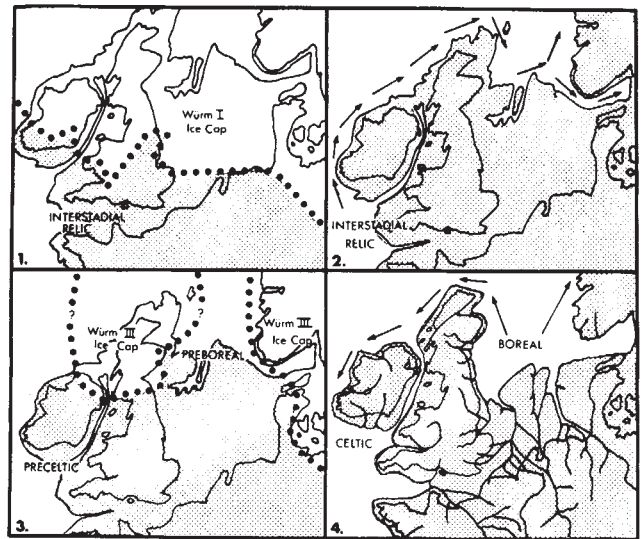


Fig. 4 Sketch map of the British Isles and the surrounding area showing geomorphological changes and the postulated migrations of the Atlantic salmon from (1) the maximum extent of Würm I Glaciation, (2) the Würm II-III Interstadial, (3) the maximum extent of Würm III Glaciation to (4) the Postglacial just before the Flandrian Transgression.

or driven south and the North British Ice Cap would have isolated the population of the Rhine Sea from the main stock extending from north-west Ireland southwards. There is no reason to suppose, however, that the isolated Rhine Sea population would not have survived.

These two populations would have been isolated genetically for at least 15,000 yr. The modern races of *S. salar europaeus* therefore are probably the progeny of these interstadial populations; the "Boreal" gamodeme having recolonized all the area vacated by the recession of the Würm III Ice Cap of northern Britain and extended its range along the west coast of Ireland. Presumably recolonization of Atlantic Scandinavia and the Baltic Sea must also have been from "Boreal" stock. On the other hand, the Celtic race has been unable to extend its range northwards. Interestingly, the present limits of the Celtic race coincide closely, though perhaps fortuitously, with the position of the terminal moraines of the Würm I Ice Cap. This possible explanation of the origin of the Boreal and Celtic races, based on the evidence of the history of the Upper Pleistocene and Postglacial in the region of the British Isles, is represented diagrammatically in Fig. 4.

We thank the commercial salmon netmen of the British Isles for assisting in the salmon sampling programme. We also thank the Fisheries Research Board of Canada for samples from the Miramichi drift net fishery (Gulf of Saint Lawrence).

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LETTERS TO NATURE

PHYSICAL SCIENCES

Variations in the 5 GHz Flux Density of the Nucleus of M87

Two recent articles^{1,2} have shown that the X-ray flux from M87 is variable over time scales of less than 1 yr. This letter reports detection of variations in the radio flux density of a compact component in the nucleus of M87, over similar time scales.

M87 has been observed at 5 GHz with the Cambridge 1 mile telescope at its maximum spacing ($2.5 \times 10^4 \lambda$) on five occasions during the past 2 yr. The first of these observations³ was part of a complete mapping in March 1969, which resolved M87 into three compact components (besides the extensive halo), the central component being coincident with the optical nucleus.

Since the other two components have large physical dimensions their contributions to the emission cannot fluctuate on a time scale of a few years and it is therefore possible to investigate small variations of flux density of the nuclear component by comparing it with the more extensive components. Fig. 1 shows a typical 12 h record of fringe amplitude and phase obtained with a spacing of $2.5 \times 10^4 \lambda$; the observed phase is plotted relative to that computed for a point source at the position of the optical nucleus⁴ α 12 h 28 m 17.572 s, δ 12° 40' 01.61" (1950.0). Because of the large east-west extent of the source, observations near meridian transit show only the unresolved nuclear component and small contributions due to the fine structure in the two outer components. The constancy of the phase at a value near zero around meridian transit demonstrates the close positional agreement between the

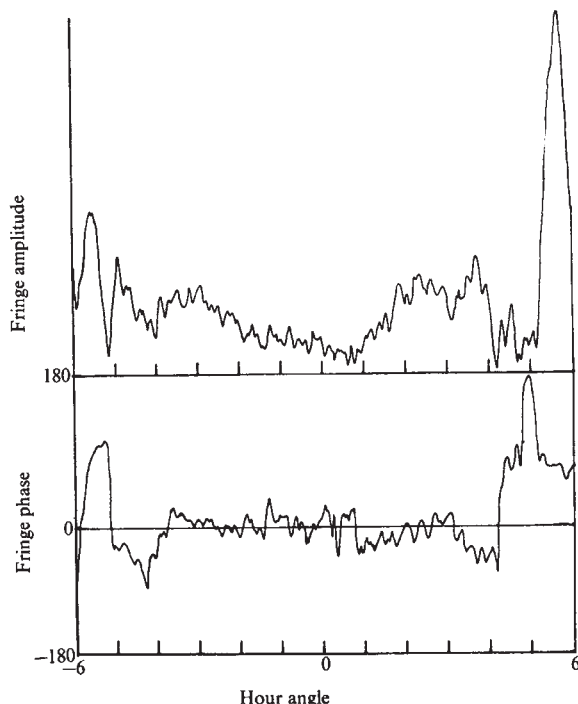


Fig. 1 Amplitude and phase plot of M87, observed at the maximum spacing of the one-mile telescope, October 20, 1970.

compact component and the optical nucleus. Numerical integration of the fringe amplitude over the range ± 01 h in hour angle therefore gives a good measure of the compact structure. In the hour angle range $+05$ h to $+06$ h the fringes lie along the major axis of the source, and the fringe amplitude is much greater due to the extensive components. A comparison of the amplitudes found for H.A. = ± 01 h and 05 h to 06 h therefore provides a measure of the flux density of the compact component, independent of the long-term calibration of the receiving system.

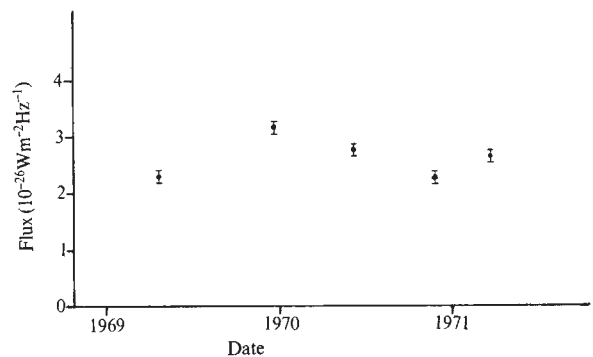


Fig. 2 Variation with epoch of the mean flux density observed in the hour angle range ± 01 h.

The results of the five observations are shown in Table 1 and Fig. 2. The ratio between the mean amplitude for the ± 01 h and $+05$ h to $+06$ h ranges has been converted to a flux density by means of a calibration on 3C309.1 on March 22, 1969, with an assumed flux density for 3C309.1 of $4.6 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}^{-1}$. The error bars shown in Fig. 2 represent the r.m.s. noise level of the telescope for a 2 h integration time and correspond to $\pm 0.10 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}^{-1}$. This figure has been derived from analysis of observations of unresolved sources. In view of the comparatively small errors the variations are believed to be significant.

Table 1 Flux Densities

Date	Flux density ($10^{-26} \text{ W m}^{-2} \text{ Hz}^{-1}$)
March 22, 1969	2.30 ± 0.10
November 19, 1969	3.13 ± 0.10
May 3, 1970	2.75 ± 0.10
October 20, 1970	2.25 ± 0.10
March 21, 1971	2.58 ± 0.10

By examining the relative amplitude and phase of the records for the observations of March 22 and November 19, 1969, between which the largest difference has been found, it is possible to set limits on the position of the variable component. If the variations occur within the nucleus no difference in phase between the two observations is expected, but if the variable component is displaced from the nucleus a progressive change in phase difference between the two records will occur. For a displacement in right ascension the magnitude of the phase difference will vary as $\cos H$, and for a displacement in declination it will vary as $\sin H$. A corresponding variation in the resultant amplitude difference will occur.

Examination of the amplitude and phase plots for the observations of March 22 and November 19, 1969, shows that the November 19 flux is consistently higher for at least 4 h on either side of transit, that is, the whole period during which the phase is approximately constant. A 90° phase difference would be produced during this period if the source of the flux variations were more than $4''$ arc from the nucleus in right ascension, or more than $20''$ arc in declination, and such a change can be ruled out. This result therefore demonstrates that the variation occurs in the nucleus, and not in the region of the optical jet.

The time scale of the variations is apparently similar to that observed for the X-rays. Unfortunately there is too little X-ray information to permit useful comparison with the radio variations. Any correlation between the X-ray and radio variations would greatly strengthen the case for the X-rays originating from the inverse Compton effect within the nucleus of M87 (refs. 3 and 5).

IAN GRAHAM

*Mullard Radio Astronomy Observatory,
Cavendish Laboratory,
Cambridge*

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Possible Upper Limit to the Distance of BL Lacertae

BL LAC (VRO 42.22.01) is a strange source of radio, infrared and visual radiation. The radio spectrum has been investigated¹ and rapid variations of radio frequency radiation have been found²⁻⁵. The continuum at visual and near infrared wavelengths has been described^{6,7} and the optical continuum is markedly polarized⁶. Rapid variations of the visual radiation on time scales as short as 1 day have recently been reported⁸.

These observations indicate that the spectrum of BL Lac is that of synchrotron radiation from non-thermal electrons, but otherwise little is known about the object, for there are no spectral lines from which the usual astronomical analyses might be done, one consequence of which is that even the distance to the source is unknown. Neutral hydrogen has been observed in absorption against BL Lac (M. Cohen, private communication), and the 21 cm line profile appears similar to other extragalactic sources. The purpose of the analysis described here is to estimate an upper limit to the distance. We recognize that there may be ways of increasing this upper limit and discuss some of these possibilities. But further data on the object are clearly necessary.

On the nights of August 25 and 27, 1970, BL Lac was observed at $\lambda = 3.5 \mu\text{m}$ and $\lambda = 11.0 \mu\text{m}$. The results of those observations were $F_\nu = 2.5 \times 10^{-27} \text{ W m}^{-2} \text{ Hz}^{-1}$ at $\nu_0 = 8.6 \times 10^{13} \text{ Hz}$, and $F_\nu = 5 \times 10^{-27} \text{ W m}^{-2} \text{ Hz}^{-1}$ at $\nu_0 = 2.7 \times 10^{13} \text{ Hz}$. These new points on the spectrum of the object are plotted with other data in Fig. 1, from which two interesting features are apparent. First, the non-thermal spectrum is flattening with decreasing frequency in such a way that most of the energy seems to be concentrated towards the region around $\lambda = 3 \mu\text{m}$, although the source has not been observed in the $\lambda = 100 \mu\text{m}$ range and a large amount of energy could be present at those very long infrared wavelengths. Second, an obvious discontinuity in the spectrum occurs between $\lambda = 2.2 \mu\text{m}$ reported by Oke *et al.*⁷ and the $\lambda = 3.5 \mu\text{m}$ flux reported here. Thus, we can conclude that the source is varying not only at radio and visual wavelengths but at infrared as well. This

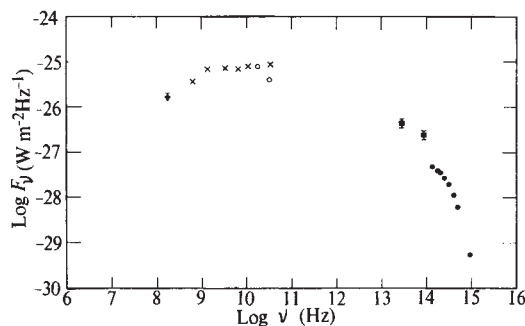


Fig. 1 The spectrum of BL Lac. References to the various measurements are given in text.

conclusion has been substantiated (G. Neugebauer, private communication) by observations of significant changes at $\lambda = 2.2 \mu\text{m}$ on time scales as short as 1 day.

We assume throughout this discussion that the spectrum of the radiation observed from BL Lac can be attributed to synchrotron radiation by non-thermal electrons in the source (which has been generally accepted). Because a large flux of Compton scattered X-rays is not observed from the source (BL Lac has not been reported as an X-ray source), the rate of loss of energy of electrons by synchrotron radiation seems to exceed the loss of Compton collisions. That is, $B^2/8\pi > U_{\text{rad}}$ where U_{rad} is the energy density of radiation in the source. Thus

$$\frac{B^2}{8\pi} > \frac{4 D^2 F_T}{R^2 c} \quad (1)$$

where D is the distance of BL Lac, R is the radius of the emitting region at wavelengths $\lambda \sim 3 \mu\text{m}$ and F_T is the total energy observed (most of which is emitted at $\lambda \sim 3 \mu\text{m}$).

This equation suggests that an upper limit to the distance can be estimated, provided that upper limits are determined for the size of the source, R , and the magnetic field, B . An upper limit on R can be set from the time scale of observed variations, $R < c\tau$. There are two methods for setting limits on the magnetic field. First, we will consider the result implied by the importance of synchrotron self-absorption for compact non-thermal sources, and second, the distance limit set by the lifetime of the radiating electrons.

Synchrotron self-absorption is important in a non-thermal radiating source when magnetic fields are large and/or when the radiation comes from a very small region. Because of the rapid variations to BL Lac implying a small radiating source and the shape of the radio spectrum, synchrotron self-absorption is probably important in this source. The flux observed at radio frequencies is given by Dent and Haddock⁹:

$$S(\nu) = 8.5 \times 10^{-34} F(\alpha) B^{-1/2} (R/D)^2 \nu^{5/2} [1 - e^{-\tau(\nu)}] \quad (2)$$

where $F(\alpha)$ is a quantity between 0.17 and 0.75 for $S(\nu) = K \nu^{-\alpha}$ and $0 \leq \alpha \leq 1$; R/D is the angular size of the source in radians and $S(\nu) (\text{W m}^{-2} \text{ Hz}^{-1})$ is the flux at a frequency ν .

In this discussion we will apply equations (1) and (2) to BL Lac. The synchrotron self-absorbing condition is used to estimate an upper limit to the magnetic field B . The resulting value is probably smaller than the actual field of the infrared source as the radio source is, if anything, larger than the infrared source. The size of the emitting region can be estimated from $R < c\tau$ where τ represents the time scale of flux variation and, as a result, an upper limit to the distance D can be calculated.

As a result of this procedure, the distance upper limit can be expressed approximately by the relation:

$$D < \left(\frac{c}{32\pi F_T} \right)^{1/10} \left(\frac{1.7 \times 10^{-34}}{S_{\text{radio}}(\nu)} \right)^{2/5} \nu_{\text{SSA}}^{1/5} R_{1R}^{1/5} R_{\text{radio}}^{4/5} \quad (3)$$

In this equation, $S_{\text{radio}}(\nu)$ and ν_{SSA} are the observed value of flux and frequency at which synchrotron self-absorption

becomes important. The sizes of the infrared and radio frequency emitting regions are specified separately as R_{IR} and R_{radio} , because the shortest time scale of observed visible and infrared flux variations is 1 day and the same quantity at radio frequencies is approximately 1 month. Applying equation (3) to the observations of BL Lac yields $D < 300$ kpc. A weakness of this method is the fact that the size of the synchrotron self-absorbed source from which the magnetic field is estimated could be larger than the fluctuating source at $\lambda \approx 3 \mu\text{m}$. A similar application of this discussion to the Seyfert galaxy NGC 1068 gives $D(\text{NGC 1068}) < 30$ Mpc, consistent with the actual distance of $D \approx 10$ Mpc.

The second method of determining an upper limit to the magnetic field involves the lifetime of the radiating electrons. The equations for the principal frequency radiated, $\nu_c = 4 \times 10^6 \gamma^2 B$ Hz, and the lifetime, $t_{1/2} = 5.3 \times 10^8 / \gamma B^2$ s, can be combined to give $B^{3/2} = 10^5 t_{1/2}$ by elimination of the γ of the electrons for radiation of frequency 10^{14} Hz. If the lifetime of the electrons were determined by a process other than synchrotron energy loss, $t_{1/2}$ would be longer than the time scale of observed variations. Thus, in general $B^{3/2} \leq 10^5 / \tau$, where τ is the observed time scale of variations. In the case of BL Lac, $\tau \approx 1$ day and therefore $B \leq 1$ gauss. Including this result in the earlier condition on Compton scattering implies $D < 1$ Mpc. Application of this method to NGC 1068 (whose distance is known) leads to conclusions similar to those discussed by Pacholczyk¹⁰, that a uniform synchrotron source is difficult to reconcile with variations on a time scale of 1 day. Further documentation of such observations is necessary.

We have applied a model of the radiation spectrum of BL Lac to the observations and found that the distance implied for this object is less than 1 Mpc, primarily because of the rapid variations observed at visual, infrared and radio frequencies from the source, implying sizes less than 10^{17} cm at radio frequencies and less than 3×10^{15} cm at visual and infrared frequencies. If variations were found to occur on even shorter time scales, this upper limit to the distance would be reduced accordingly. Variations of $\leq 10\%$ are found on shorter time scales¹¹, although variations in flux values of two, as discussed here, have not been found on these shorter time scales. More energy at longer infrared wavelengths would only increase the radiation energy density in the source and lower the upper limit estimate of the distance given here. The possibility that the discussion is not valid because of relativistic expansion in the source must be considered. Thus, our derived distance result is a "possible" upper limit. It must also be acknowledged, however, that relativistic expansion has not been observed in any extragalactic source for which the distance is known. In fact, observations of 3C 120 and NGC 1275 indicate definite non-relativistic expansion. Also, the possibility that the ratio of Compton scattered energy to synchrotron energy is greatly increased because electrons travel along the lines of force is unlikely for BL Lac because of the large percentage of polarization observed from this source.

We hope that the result derived here will encourage further observations of BL Lac. The time scale of variations at all wavelengths should be determined and the possibility that there is radiation with $\lambda = 100 \mu\text{m}$ from the source should be investigated. The object will probably appear as an X-ray source at some flux level and an effort should be made to detect it.

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University of California, San Diego,
and University of Minnesota

University of California, San Diego

Lick Observatory, University of California

W. A. STEIN

F. C. GILLET

R. F. KNACKE

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Visual, Photographic and Photoelectric Detection of Shadow Bands at the March 7, 1970, Solar Eclipse

Just before and after the total phase of a solar eclipse, faint light and dark bands, resembling interference or diffraction fringes, move across the surface of the Earth. These "shadow bands" were first reported as early as 1842 (ref. 1). Work by Feldman² indicated that the orientation of the bands was related to the shape of the shadow on the surface of the Earth, but many of the data were difficult to interpret. Paulton³ has described a screen with which visual observations can be made with some degree of accuracy and consistency, and a screen of this type was used by two of us (M. E. H. and R. D. B.) at the eclipse of November 12, 1966, in Rio Grande do Sul, Brazil⁴. Paulton⁵ has reviewed the visual work done at the 1963 and 1966 eclipses.

In our experiments, five screens to Paulton's design¹ (Fig. 1) were oriented with their surfaces perpendicular to the Sun's rays and the 0° - 180° line aligned in a north-south direction with 0° to the north. Three of the screens were set up on the centre line of the path of totality in Greenville, North Carolina, and the three groups operating these three screens will be referred to as Nos. 1, 2 and 3. Group 4 was located a perpendicular distance of 26 miles to the south-east of the centre line at Vanceboro, North Carolina, and group 5 was located a perpendicular distance of 26 miles to the north-west of the centre line near Suffolk, Virginia. These locations were within the

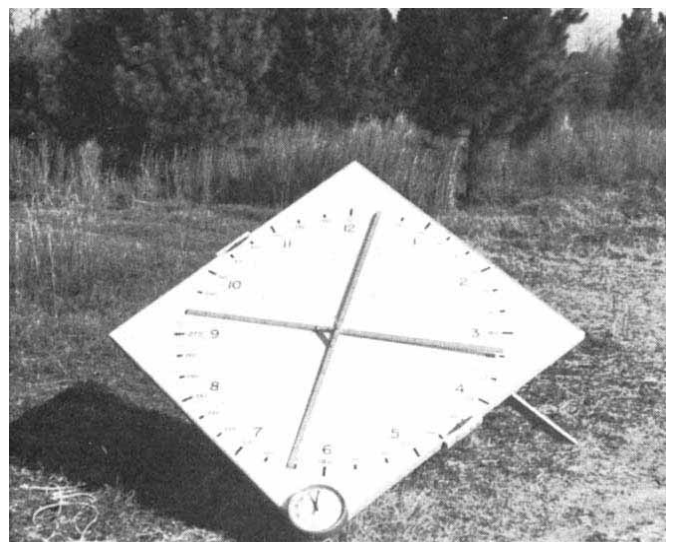


Fig. 1 Shadow band screen with the 0° and 180° marks at opposite corners and mounted on an equatorial mount (photograph by D. A. M.).

path of totality which was approximately 85 miles in the North Carolina–Virginia region.

The orientation of the shadow bands was determined by rotation of the cross stick on the screen so that one stick was parallel to the bands. The angular position was read off the 0°–360° clockwise scale. Groups 1, 3 and 5 used screens lying flat on the ground (with 0° to the north) as well as screens perpendicular to the Sun's rays. Orientation was read in the same way and the readings were translated geometrically for comparison. The velocity of the shadow bands was determined by starting a stopwatch when a band first appeared at one edge of the screen and stopping it when the band reached the other edge. The spacing of the bands was estimated by comparing the bands with the 10 cm marks on the stick which was perpendicular to the orientation of the bands. Accurate timing was obtained by recording all visual events verbally on a cassette tape recorder.

The low light level, low contrast and rapid motion make it difficult to photograph shadow bands. Experiments using simulated shadow bands in the laboratory indicated that Kodak recording film No. 2475 (ASA 1000, black and white), forced developed to a speed of ASA 4000, would be best. This film was used in a 35 mm Miranda F camera (50 mm, $f/1.9$ lens) at an exposure of 1/125 s. The following photographic techniques were also used: (1) ANSCO D 200 film, ASA 200 (colour), developed to ASA 500, using a 16 mm Bolex motion picture camera (16 frames s^{-1} giving an exposure of about 1/35 s); (2) Kodak Tri-X film, ASA 200 (black and white), using an 8 mm (super 8) Technicolor Mark Two movie camera (16 frames s^{-1}); (3) Polaroid Type 3000 film using a Polaroid Model 100 automatic camera. The 35 mm still photography recorded the shadow bands but little could be determined from microdensitometer traces because of the grain of the film. Methods 1 and 2 also revealed the shadow bands when the films were projected but gave no new information. The Polaroid method produced photographs which were considerably overexposed.

We also used the following photoelectric systems: (1) Welch Densichron system in conjunction with a Varian G-14A strip chart recorder; (2) an unfiltered photo diode connected directly to an Esterline Angus strip chart recorder (Model S601S); (3) a photo diode with an H α line filter (≈ 100 Å bandwidth) connected to an Esterline Angus strip chart recorder (Model E1101S); (4) six photo diodes each covered with a specific narrow band filter with maximum transmission approximately at a selected flash or Fraunhofer spectra line. Each photo diode was connected to a chopper, a preamplifier and a cassette tape recorder. The selected wavelengths were 4861 Å, 4340 Å, 3968 Å, 5279 Å, 5896 Å and 6870 Å.

Row 1 in Table 1 gives the experimentally determined orientations of the shadow bands as observed on the screens perpendicular to the Sun's rays, both before and after totality at all three locations. At two locations, Greenville and Suffolk, readings on screens in a horizontal position were also taken, and row 2 gives values obtained in this way. Row 3 gives the geometrical tangents to the shadow cone at the three locations both before and after totality. By comparing the three values in each column it is clear that on the centre line of the eclipse path

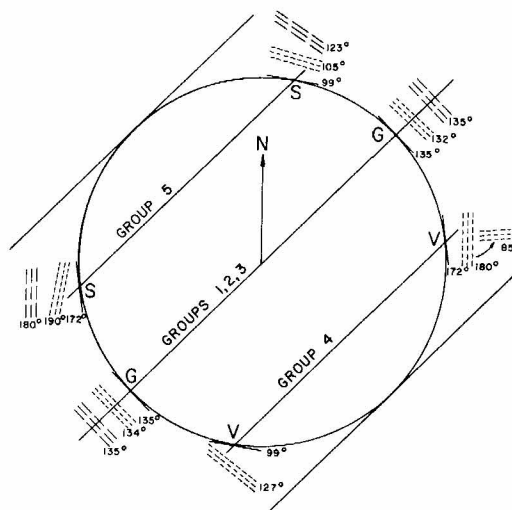


Fig. 2 Comparison of experimental orientations of shadow bands with geometrical tangents to the shadow cone for a surface perpendicular to the Sun's rays. —, Geometrical tangents; ---, experimental orientations; - · -, horizontal readings, translated.

at Greenville the shadow bands are tangential to the shadow cone, although 26 miles off the centre line in both directions they are somewhat off tangency. A curious rotation of the bands occurred at Vanceboro during the minute or so before totality. They rotated from an orientation of 180° counter-clockwise to a position of 85° just before totality. This rotation and the fact that the bands are off tangency at Vanceboro and Suffolk indicate that the shadow bands may involve a combination of physical phenomena. The comparison of the experimental values and geometrical tangents is shown in Fig. 2. Table 2 and Fig. 3 compare the experimentally determined orientations if the shadow bands in a horizontal plane and the orientations on a plane perpendicular to the Sun's rays translated to the horizontal plane, with the geometrical tangents to the elliptical shadow on the surface of the Earth.

The reported values of the spacing of the bands varied widely. For example, group 2 reported 15 to 20 cm for the centre to centre separation of the shadow bands, although group 3 reported 7.5 to 8 cm for the same distance. The actual width of the dark portion of the bands was estimated as 1 cm by group 2, 2.5–3 cm by group 3, and 3 cm by group 4, changing to 2 cm as totality approached. Group 3 also reported a decrease in the width of the dark portion as totality approached.

The shadow bands in Greenville moved up the screen, both before and after totality, at an angle of approximately 45°, that is, the bands first touched the screen at the 225° position and left the screen at the 45° position. The fact that the bands moved up the screen in Greenville both before and after totality, combined with the fact that the shadow itself was moving several orders of magnitude faster than the shadow bands, showed that the bands moved toward the shadow before totality and away from the shadow after totality. At Vance-

Table 1 Experimentally Determined Orientations of Shadow Bands in a Plane Perpendicular to the Sun's Rays compared with the Geometrical Tangents to the Shadow Cone

	Greenville		Vanceboro		Suffolk	
	Before totality	After totality	Before totality	After totality	Before totality	After totality
Experimental orientations	$132^\circ \pm 5^\circ$	$134^\circ \pm 5^\circ$	$180^\circ \pm 5^\circ$	$127^\circ \pm 5^\circ$	$105^\circ \pm 5^\circ$	$10^\circ \pm 5^\circ$ or $190^\circ \pm 5^\circ$
Experimental orientations ^a	$135^\circ \pm 5^\circ$	$135^\circ \pm 5^\circ$	No report	No report	$123^\circ \pm 5^\circ$	$1^\circ \pm 5^\circ$ or $181^\circ \pm 5^\circ$
Geometrical tangents to shadow	135°	135°	172°	99°	99°	172°

^a Experimental results on horizontal screens geometrically translated to a plane perpendicular to the Sun's rays.

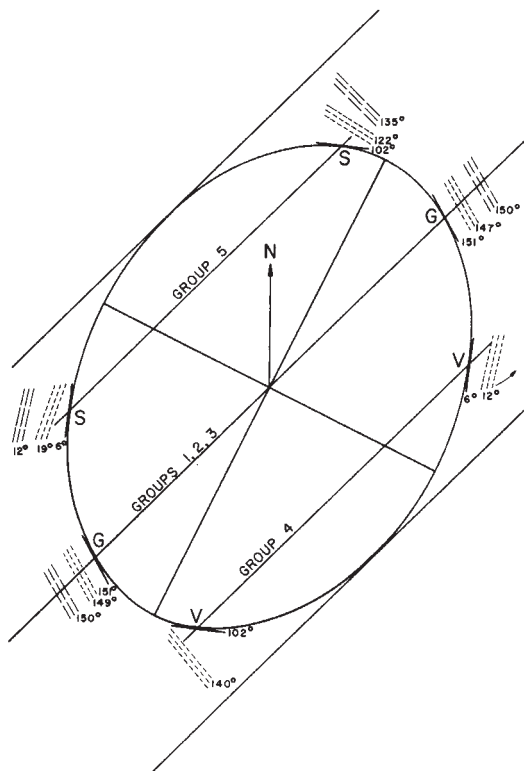


Fig. 3 Comparison of experimental orientations of shadow bands with geometrical tangents to the shadow cone for a horizontal surface. —, Geometrical tangents; ---, perpendicular readings, translated; — — —, experimental orientations.

boro and Suffolk the readings also indicate motion toward the shadow before and away from the shadow after totality.

Velocity estimates also varied somewhat among the observing groups; group 1 estimating 2 m s^{-1} both before and after totality, groups 2 and 4 estimating 3 m s^{-1} before totality, and group 3 reporting that during the minute or so of shadow band activity before totality the velocity of the bands increased by a factor of two or three.

Fig. 4 shows the results using the unfiltered photo diode connected directly to a high speed strip chart recorder. Fig. 4a shows very little activity 7 min 45 s before totality. Note the rapid decrease of the total illumination towards totality. Figs. 4b and c show definite shadow band activity at 96 s and 32 s, respectively, before totality. Note that the shadow bands were detected photo-electrically before visual sightings were made. Visually the bands were first detected 70 s before totality. Because of the rapid decrease in overall illumination Figs. 4b and c give a "step" appearance where there are small variations in the signal, making it difficult to identify a definite pattern. From the larger signals there seems to be a frequency of 4–5 Hz, but considering both the large and small signals there seems to be a mixture of frequencies ranging from perhaps 4–30 Hz. Fig. 4d shows a decrease in activity 16 s before totality, but this may indicate decreased sensitivity to light variations at

lower illuminations. The other photoelectric systems did not perform well enough to give reportable results. When the tapes were played back and strip chart records were made there was too much noise for any signal to be detected.

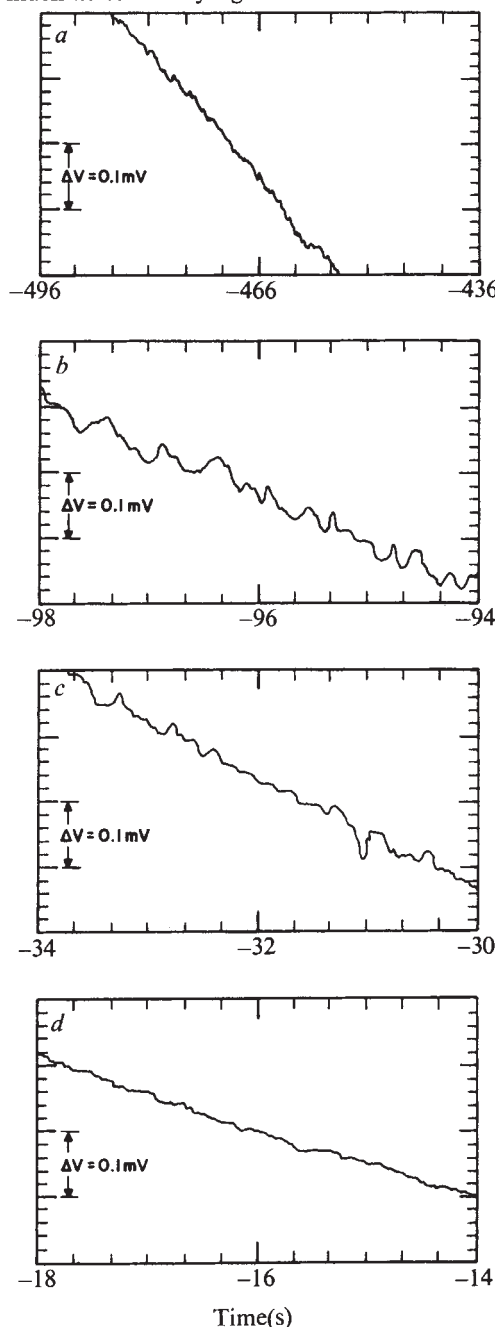


Fig. 4 Photoelectric strip chart records of shadow bands: (a) 7 min and 45 s before totality showing very little activity; (b) and (c) 96 s and 32 s, respectively, before totality showing definite shadow band activity; (d) 16 s before totality showing practically no signal.

Table 2 Experimentally Determined Orientations of Shadow Bands in a Horizontal Plane as compared with the Geometrical Tangents to the Shadow Cone

	Greenville		Vanceboro		Suffolk	
	Before totality	After totality	Before totality	After totality	Before totality	After totality
Experimental orientations	$150^\circ \pm 5^\circ$	$150^\circ \pm 5^\circ$	No report	No report	$135^\circ \pm 5^\circ$	$12^\circ \pm 5^\circ$
Experimental orientations ^a	$147^\circ \pm 5^\circ$	$149^\circ \pm 5^\circ$	$12^\circ \pm 5^\circ$	$140^\circ \pm 5^\circ$	$122^\circ \pm 5^\circ$	$19^\circ \pm 5^\circ$
Geometrical tangents to shadow	151°	151°	later rotated 6°	102°	102°	6°

^a Experimental results on screens perpendicular to the Sun's rays geometrically translated to the horizontal plane.

To sum up, on the centre line of the path of the eclipse the shadow bands were at a tangent to the eclipse shadow both before and after totality. At locations off the centre line, but still within the path of totality, the shadow bands were only roughly tangential to the eclipse shadow and, in one case, they rotated considerably during the approximately 1 min period of shadow band activity before totality. Shadow bands tangential to the shadow might suggest some type of diffraction as the cause, but with the variations described this is certainly in question. Perhaps there are two or more competing causes. We estimated that the centre to centre spacing of the bands was approximately 10 cm, and from photoelectric results we suggest that the shadow bands came at frequencies varying from 4 to perhaps 30 Hz with a corresponding variation in spacing. The velocity of the bands seems to be $2\text{--}3\text{ m s}^{-1}$. Relative to the elliptical shadow itself (moving at about $1,000\text{ m s}^{-1}$) the bands moved toward the shadow before totality and away from the shadow after totality.

We hope to have developed improved techniques in time for the eclipse of July 10, 1972, in Nova Scotia.

S. Hulst, R. Hulst, C. Hulst, S. Morrow, Dr N. Sprague, Professor J. Vidal (visiting lecturer from Pelotas, Rio Grande do Sul, Brazil), Ronald Presdorf, Rita Presdorf, T. Rettig, N. Rettig, and R. Hackeney made visual observations. J. Hulst, B. Warn, and Lloyd and Laura Mitchell carried out photographic work. D. Puntunney helped with the photoelectric measurements. This project was supported by the Atmospheric Sciences Section, US National Science Foundation.

MALCOLM E. HULST
ROGER D. BURGESS
DANIEL A. MITCHELL
DUANE W. WARN

Department of Physics,
Ball State University,
Muncie, Indiana 47306

Received June 22, 1970; revised April 5, 1971.

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Meteorological Phenomenon called Crown Flash

Gall and Graves¹ described a ground observation of a sudden brightening effect around the top of an active cumulonimbus cloud. Each brightening began concurrently with a lightning stroke in the main cloud mass and rippled upward and outward during a substantial fraction of a second.

Subsequent to this report, brief prior discussions of unusual optical phenomena of this kind have been noted in the literature²⁻⁴. These observations deal with the sudden disappearances of apparent cloud streamers which sometimes seem to reform. References to earlier "flachenblitz" observations imply that such reports may have a long history.

A very tenable explanation of this class of visual sighting was proposed by Vonnegut⁵ in terms of changes in orientation of ice crystals, with their mirror-like reflexions, around the crests of thunder clouds. Such changes are postulated to occur when a relatively strong electric field induces electric dipoles in the ice crystals, with their resulting tilting forces. For example, if the visual effect is a streamer disappearance, a rapid, lightning-induced change in ice crystal orientation has taken place, destroying a slowly built up electric field and ice crystal alignment.

Two types of special observations carried out by Vonnegut⁵ support this explanation. First, a laboratory cold box experiment duplicating the conditions described confirmed the



Fig. 1 An illustration of the crown flash, taken from ref. 1.

transitory nature of reflexions from ice crystals in a changing electric field. Second, photo-tube measurements of light changes in solar-illuminated cumulonimbi showed that both increases and decreases can occur at the time of lightning flashes.

The report by Gall and Graves seems to be accounted for by a momentary increase in brightness, which progresses away from the cloud top as the ice crystals change their orientation, passing through an angle of tilt giving optimum reflectance. Even when the brightening appears to extend beyond the cloud streamer into "blue sky" (as noted by them during their observation) the presence of invisible cirrus could account for this effect. As was pointed out by Vonnegut⁵, such visual sightings may provide useful information concerning the electrical processes associated with thunderstorms.

MAURICE E. GRAVES
JOHN C. GALL, JUN.
BERNARD VONNEGUT

Department of Meteorology and Oceanography,
University of Michigan,
College of Engineering,
Ann Arbor, Michigan 48104

Received April 19, 1971.

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Persistent Water Vapour Contamination and Stratospheric Measurement

WATER vapour profiles in the stratosphere have long been a subject of controversy because of the difficulties involved in measuring extremely small water vapour concentrations. Such information is important both intrinsically and because of the influence of water concentration on the radiation budget of the Earth. But the determination of these profiles is expected to arouse even greater interest and activity because of the

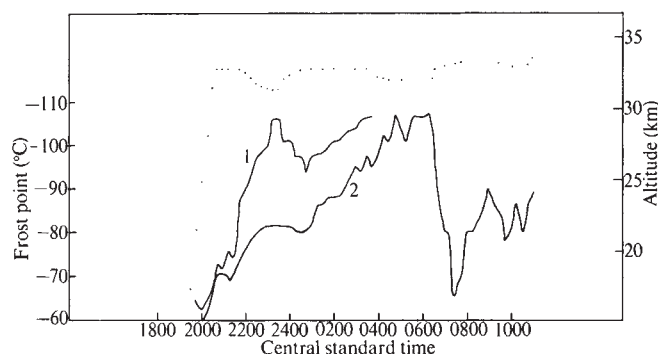


Fig. 1 Altitude and frost point data during flight launched 1831 CST on September 21, 1970. Curve 1 represents data received from a sensor hanging from an 8 m cable, curve 2 from a sensor positioned at the end of a 1 m boom. Individual altitude points are shown by dots.

possibility that injection of large amounts of water into the stratosphere by a fleet of supersonic transports could radically alter the water concentration in the stratosphere with the consequent severe ecological effects. We present here evidence of the subtle and long lasting effects of water contamination sources on ambient water vapour measurements in the stratosphere. This evidence was obtained during a long balloon flight at a float altitude of approximately 31–33.5 km.

All surfaces can adsorb and desorb water and as such are to be avoided if the true ambient water vapour concentration is to be measured¹. Apparently negligible water contamination concentrations were present only during descent from maximum balloon altitudes. It is generally considered, however, that desorption of water vapour at reduced pressures (≈ 10 mbar) is not a problem after several hours exposure to low ambient water concentrations. The experimental results discussed here indicate that such expectations are not fulfilled.

The instrumentation used was the aluminium oxide water vapour sensor², which is a capacitor composed of a thin layer of aluminium oxide sandwiched between two electrodes, one a base aluminium sheet and the other a vapour-deposited gold overlay. The sensor responds to water vapour pressure alone and is temperature-independent. The sensor is passive: it does not require any sampling system with the accompanying water contamination sources present in the tubing of the sampling system. Because the sensor is a transducer, its output as a function of dew/frost point must be determined by a calibration procedure³.

The sensor has been used for meteorological measurement of water vapour profiles during balloon flights up to altitudes of approximately 30 km. Some preliminary results showing descent values have been reported together with a laboratory evaluation of the sensor performance^{4,5}. The equipment being used is considerably modified from that described earlier. Also, all possible contamination sources have been eliminated from the vicinity of the sensor so that protective cages and fittings have been eliminated and this "nude" sensor is currently suspended from a radiosonde at the end of an 8 m cable to minimize further the effects of contamination. We conducted a long duration, high altitude flight using the latest versions of the aluminium oxide sensor and circuitry. Two such sensors were installed, one hanging from the end of an 8 m cable and the other installed at the end of a boom extending out about 1 m from the enclosure containing the battery supply and telemetry equipment. This latter method of mounting is identical to that used during the preliminary flights⁵. The instrument enclosure was suspended about 32 m below the balloon. The balloon itself had a volume of 2×10^7 l. and was launched from Palestine, Texas, at 1831 CST on September 21, 1970. The flight was terminated at Pecos, Texas, at 1100 CST on the following day. The total float duration was slightly more than 14.5 h.

The altitude and dew/frost point data received as a function of time are shown in Fig. 1. Neither complete ascent data nor descent data are shown (as the boom sensor was damaged at the end of the float). Because an ice layer formed on the sensor during ascent, only less than saturated frost point data are shown in Fig. 1. Data were received from both sensors during approximately the first half of the flight only, data reception problems being encountered for the hanging sensor during the latter part of the flight.

The data clearly show the long duration of the effects of contamination. The average steady-state frost point during float was about -105°C . Although float altitude was achieved at 2028 CST, the steady state value of frost point was not indicated by the hanging sensor until about 2.5 h later. The sensor on the boom, which was closer to the contamination sources (both the balloon and instrument enclosure), showed a steady decrease in frost point readings, approaching those of the hanging sensor, but did not reach the steady state value until about 8 h after float altitude was reached. The behaviour of the data from the hanging sensor after the steady state value was achieved is interesting. An increase in frost point reading coincides with the release of ballast from the balloon at 2331 CST. The balloon had been floating at an altitude of about 31.4 km until then, after which it ascended to 32.8 km, reached at 0056 CST. Even though the ascent rate was slow, both sensors were subjected to water contamination contained in the wake of the balloon. The sensor situated on the boom was still influenced principally by the contamination from the instrument enclosure so that the predominant indication of the balloon ascent in this period is an interruption in the rate of decrease in its indicated frost point reading. The hanging sensor, however, clearly indicates an apparent increase in indicated frost point of more than 10°C , implying an order of magnitude increase in water vapour concentration. When the new float altitude of 32.8 km was reached, both sensors indicate the decreasing contamination concentration.

The most significant feature of the data shown in Fig. 1 is the large increase in measured frost point which commences at 0618 CST, 5 min after sunrise at this location and altitude. For more than 1 h before 0618 CST, the balloon had been floating at an altitude of 32.1 ± 0.1 km and was still at this altitude at 0628 CST. The balloon then ascended rapidly to 32.7 km at 0636 CST and more slowly to 33.2 km at 0745 CST. This ascent was not caused by a release of ballast. Because sunrise occurred at 0613 CST, we consider that the effects observed are principally the result of direct incidence of light from the Sun causing both an increased rate of water desorption from the balloon and instrument enclosure and, somewhat later, balloon ascent. The total indicated increase in water vapour concentration is more than three orders of magnitude. It is unlikely that changes in ambient temperature or the incidence of the Sun on the sensor caused the effect noted, because either would be inconsistent with the general decrease in contamination concentration after 0730 CST. Although balloon ascent also ceased at approximately this time, for the remainder of the flight the measured water vapour concentration remained about two orders of magnitude greater than it had been before sunrise.

It should be noted that the minimum contamination, steady state frost point measured by both sensors, about -105°C , corresponds to a mixing ratio of $0.4 \times 10^{-6} \text{ g g}^{-1}$ at a pressure altitude of 8 mbar, which is lower than the range of $1\text{--}4 \times 10^{-6} \text{ g g}^{-1}$ reported by Mastenbrook¹. An overlap can be achieved, however, if the frost point is increased by an uncertainty factor of about $\pm 5^\circ\text{C}$ estimated by allowing for both sensor calibration curve and telemetry errors for very low frost point readings. These results are therefore in agreement with a "dry sky" model of the stratosphere.

Many stratospheric water vapour measurements are suspect because of the possibility of contamination during balloon ascent^{1,6}. These results indicate that the problem may have

been underestimated because, even after floating at 32 km or higher for 10 h, sufficient water vapour contamination was present to increase the concentration by three orders of magnitude. Furthermore, the contamination remained at an intolerably high level after 14.5 h of float. This contamination has significance for stratospheric water vapour measurements and for all instruments and vehicles (aircraft and rockets as well as balloons) which, of necessity, contain water adsorbing surfaces. Such surfaces must be minimized as they may evolve water, particularly when irradiated by energy from the Sun. Contamination is also significant for measurements of other stratospheric parameters which may be affected by water vapour interferences, such as refractive index and ozone concentration.

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PHILIP GOODMAN
DAVID CHLECK

Panometrics Inc.,
Waltham, Massachusetts 02154

Received March 22, 1971.

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Deficiency of Gravity Corers for sampling Meiobenthos and Sediments

GRAVITY CORERS are widely used^{1,2} for the collection of the smallest marine metazoans (meiofauna) from subtidal grounds, and it has been generally accepted that cores which appear undisturbed give reasonably representative samples. But, after comparing gravity cores with samples collected by SCUBA divers, I suggest that this is not so.

Comparisons were made as a result of observations by divers on a muddy bottom at a depth of 30 m in Loch Ewe on the west coast of Scotland. It was difficult to collect all the flocculent material at the mud-water interface using core tubes of about 2 cm diameter however carefully the tube was inserted, even when the diver was steadied by a permanent frame support on the bottom. Similar difficulties were encountered with tubes of 4 cm diameter, and it was only when a considerably larger size (10 cm diameter) was used that the problem seemed to have been overcome. Divers were able to insert these large diameter tubes about 15 cm into the deposit and seal them with a rubber stopper fitted with a valve, without causing any observable disturbance of the sample surface. The tubes could then be withdrawn, sealed at the lower end, and returned to the ship apparently undisturbed. After carefully siphoning off the supernatant water to a few cm above the mud surface, it was possible to extract undisturbed cores in tubes of about 2 cm diameter—a convenient size for analysis of the fauna.

To assess the technique and to compare it with previous results from gravity corers, a series of samples collected in 10 cm tubes by divers, and subsampled as described in 2.2 cm tubes, was compared with another series collected using a light-weight gravity corer³ (17 kg in air) fitted with a 2.2 cm liner. Collec-

Table 1 Organisms retained in the Sieve

Month	Nematodes		Copepods		Others	
	Diver core	Gravity core	Diver core	Gravity core	Diver core	Gravity core
January	240	169	35	4	20	18
May	386	257	101	9	63	50
June	877	482	123	34	90	48
October	865	641	54	23	33	12

tions were made in several months of the year to check the consistency of results in varying conditions. The top 6 cm of mud from each core was used because it has been shown that most of the fauna was concentrated in this layer. Samples were preserved in 5% formalin, and screened through a mesh of 37 μ m. All the organisms retained in the sieve were counted, and comparisons of individual pairs of gravity and diver cores from different months are shown in Table 1. Nematodes and copepods, which made up about 90% of the fauna, are shown separately and all other organisms (mainly turbellarians, gastrotrichs and kinorhynchans) are grouped together.

The data, which represent the range of variation found throughout the year, show that the diver cores consistently gave substantially higher counts than the gravity samples. This applies to the copepods in particular, and it can be shown, by dividing cores into segments 1 cm thick, that these animals are largely restricted to the top 1 cm of mud. These results support the suggestion that the downwash caused by gravity corers in their descent disperses a substantial proportion of the superficial layer of deposit and that such instruments thus fail to collect all the animals associated with this layer. Most previous assessments of meiofauna (and especially copepods) by methods of this type may therefore be substantially underestimated. Furthermore, the deficiency of these instruments (or of any instrument dropped, however gently, onto the sea bed) in collecting intact the superficial layer of sediment should be recognized in selecting sampling techniques to study recently settled organic matter or pollutants which might be concentrated at the mud-water interface. A detailed report on this work is in preparation.

A. D. McINTYRE

Marine Laboratory,
Aberdeen

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BIOLOGICAL SCIENCES

Effects of Marihuana on the Solution of Anagrams, Memory and Appetite

ALTHOUGH anecdotal opinions on the effects of marihuana (*Cannabis sativa* L.) are common, little objective information is available. The following study is concerned with the effects of marihuana on memory, simple intellectual performance, and hunger in humans.

Thirteen adults aged between 22 and 37 served as volunteer subjects. In the marihuana condition there were three men and four women, all of whom had previously had several experiences with marihuana. Six of them, two men and four women, served as controls. All subjects knew beforehand whether they would be given marihuana. The experiment was run in two parts. The conditions for part A were the same for both marihuana and control subjects. Part B differed only

in that one group received marihuana before being tested. Each group was tested separately with two to three subjects per test session. All subjects had eaten a meal 2 h or less before being tested.

Before the experiment began, and at least 30 min before any subjects were given marihuana, each subject was given a plate of marshmallows, "in case someone might get hungry during the experiment". Marshmallows were chosen in the absence of any evidence that marihuana affects preference for this food. At the end of each session, all plates were refilled and, without the knowledge of the subject, a record was kept of the number of marshmallows eaten by each individual during the session.

marihuana, although small, was a significant decrease ($U=14$, $P<0.032$).

This effect on memory was corroborated by data from the Babcock test. Three judges were given the recall protocols and asked to rate them for the number of "idea units" based on a prescored protocol which they kept before them. Protocols were also rated for number of errors after the procedure outlined by Rapaport² and for the number of content words which appeared in the original. Control subjects recalled significantly more idea units ($U=3$, $P<0.004$) and content words ($U=0$, $P<0.001$) than did subjects who had smoked marihuana. The fact that the groups did not differ significantly in the total number of words used in their protocols

Table 1 Effects of Marihuana on the Solution of Anagrams, Memory and Appetite

Treatment	Mean time to solve anagrams	Mean recall of anagram items per subject	Mean recall of idea units per subject	Mean recall of content words per subject	Mean total words per subject	Mean No. errors per subject	Mean consumption per subject
Part A (Premarihuana condition)							
C	720 s	4.2					34.5
M	630 s	4.3					20.9
Part B (Marihuana condition)							
C	516 s	4.2	8.5	9.6	28.5	7.2	3.8
M	596 s	3.3	3.5	5.3	24.1	10.1	45.6

At the beginning of part A each subject was tested on the Taylor manifest anxiety scale (MAS)¹. The results of this study will be reported elsewhere. After the questionnaire had been administered, the subjects took part in an anagrams game. First, the experimenter demonstrated that if the five letters on a 5×3 inch card were unscrambled, they would form a recognizable word. The subjects' task was to unscramble the letters in the shortest possible time. Answers were to be written and circled on a pad with the correct solution indicated by a verbal signal such as "OK". A 3 min time limit was given per word and subjects were told that a record would be kept of their solution times. After the last anagram had been tested, all the answer slips were collected and subjects were asked to write down as many of the correct words as they could remember.

Part B followed immediately after the return of the marshmallows to each subject. Subjects were then asked to listen to a "newspaper report", which was in fact a copy of the Babcock memory test described by Rapaport². Subjects in the marihuana condition were then given two marihuana cigarettes to smoke. This took approximately 5 min. Control subjects received no comparable treatment and were allowed to rest for this brief period. Ten minutes after the last cigarette had been smoked, the MAS was re-administered. After its completion, subjects were asked to write out as much of the newspaper story as each could remember. The rest of the experiment was exactly like that described for part A except that a different set of anagrams was used.

The data, presented in Table 1, were evaluated using the Mann-Whitney statistic³. There were no significant differences between groups in the time to solve the anagrams in either parts A ($U=19$, n.s.) or B ($U=23$, n.s.). This result supports Williams's⁴ finding that pyrahexyl, a marihuana-homologue, had no effect on performance on the Wechsler-Bellevue intelligence test.

There were no significant differences in the recall of items from the anagrams list in part A. In part B, however, subjects who had smoked marihuana recalled significantly fewer items than did controls ($U=11$, $P<0.054$). Inspection of the table indicates that the mean ($\bar{X}$) number of recalled items was identical in both sessions for controls. On the contrary, the change from an $\bar{X}$ of 4.3 to 3.3 items per subject, after smoking

($U=16$, not significant) indicates that the greater number of ideas and content words correctly recalled was not a result of control subjects simply writing more than marihuana subjects. Comparison of the error scores also indicates that not only do marihuana subjects remember less than controls, but what they do recall is likely to be less accurate ($U=7$, $P<0.026$). Abel⁵ has reported that the smoking of marihuana adversely affects the recall of prose material. In that study, however, the material was visually presented. In this study, the generality of this earlier finding is extended by using both visual (anagrams) and auditory (newspaper story) means of presentation.

As for the effects of marihuana on hunger, control subjects ate significantly more marshmallows in part A than did subjects who were given marihuana later ($U=9$, $P<0.03$). In part B, this relationship was reversed—marihuana subjects consumed significantly more marshmallows than controls ($U=4$, $P<0.004$). Five individuals in the marihuana condition consumed more marshmallows in B than in A. None of the subjects in the control group did so. Although it is possible that control subjects were satiated in part B, it is unlikely that subjects in the marihuana condition would significantly have increased their consumption of marshmallows had marihuana had no effect on some hunger-related variable. It therefore seems reasonable to conclude that marihuana increased the subjects' desire for food, a finding which corroborates the experimental findings of Abel and Schiff⁶ with respect to rats.

This study was conducted while I was a postdoctoral fellow at the University of California at Berkeley.

ERNEST L. ABEL

Department of Psychology,
University of Toronto

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Electron Energy Loss Spectra of the Nucleic Acid Bases

As part of a programme to investigate the interactions of fast electrons with important biological molecules, we have measured the energy loss spectra of ~ 20 keV electrons transmitted through thin films of five nucleic acid bases. We measured these spectra in order to determine whether the energy loss information could be used as a contrast mechanism in high resolution scanning transmission electron microscopy¹. But the spectra can also provide information about atomic, molecular, and solid state transitions in the range ~ 1 eV to 10^3 eV. Furthermore, changes in the spectra as a function of irradiation can provide a measure of the radiation damage sustained by the molecules due to the action of the incident electron beam.

All five nucleic acid bases were purchased from Calbiochem, and used without further purification. Films about 500–1,000 Å thick were deposited onto ~ 30 Å thick carbon substrates by vacuum sublimation. A 'Pyrex' crucible containing the powdered sample was heated slowly in an ion-pumped system maintained at $< 1 \times 10^{-7}$ torr. To reduce the possibility of thermal decomposition, sublimation was carried out at the coolest possible temperature^{2,3}.

Energy loss was measured at room temperature in a vacuum of $\sim 5 \times 10^{-10}$ torr using an electron gun scanning microscope with a high resolution magnetic spectrometer. The microscope has been described before⁴ and consists of a field emission tungsten tip and a specially designed electron gun¹. The focusing action of the gun is such that a 100 Å diameter spot with a current of $\sim 10^{-10}$ A can be formed at the specimen without the aid of auxiliary lenses. The electron beam has a total divergence angle of 1 mrad at the specimen.

The spectrometer is a uniform field sector magnet with curved pole faces which correct spherical aberration in the momentum dispersion plane⁵. The measured resolution of the spectrometer is 50 mV out of 25 kV and the resolution of the entire system is limited only by the ~ 0.2 eV spread of the electrons from the field emission source.

Electrons transmitted through the specimen with a given energy loss as selected by the spectrometer are detected by a scintillator-photomultiplier combination and then counted and stored in a 400 channel analyser. All spectra were taken using those electrons which were scattered into a 1 mrad cone centred about the zero scattering angle.

To minimize radiation damage, the total dose per spectrum was held to $< 10^{-3}$ Coulombs/cm². To establish that this dose did not significantly damage the specimen, we made measurements with doses ten to twelve times smaller and found no change in the spectra. Furthermore, significant changes in the spectra were noticed only for doses $> 10^{-2}$ Coulombs/cm². Because the microscope operates at a vacuum of $\sim 5 \times 10^{-10}$ torr, specimen contamination is negligible.

As Fig. 1 and Table 1 show, all the spectra have a somewhat similar structure, consisting of three or four peaks and

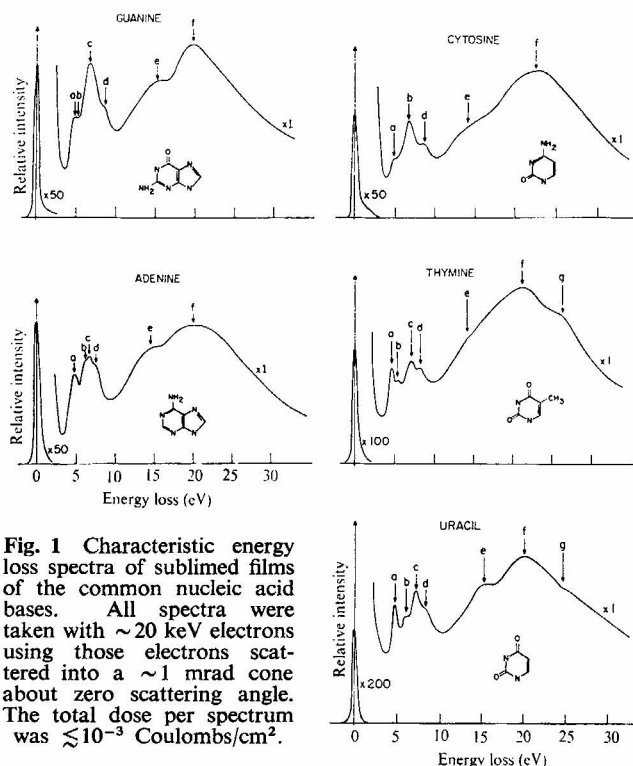


Fig. 1 Characteristic energy loss spectra of sublimed films of the common nucleic acid bases. All spectra were taken with ~ 20 keV electrons using those electrons scattered into a ~ 1 mrad cone about zero scattering angle. The total dose per spectrum was $\lesssim 10^{-3}$ Coulombs/cm².

shoulders in the region below 10 eV, a definite shoulder at about 15 eV, a relatively broad maximum at around 20 eV and, in some cases, small shoulders around 25 eV.

The low lying structure below 10 eV can be correlated with existing data³ on the ultraviolet absorption of thin films of these bases. A comparison of our energy loss measurements with the ultraviolet absorption data is shown in Table 1. An exact correspondence of the maxima of the two spectra is not expected, for the intensity distribution in the energy loss spectra is proportional to $\text{Im}(1/\epsilon)$ whereas in the optical absorption spectra it is proportional to $\text{Im}(\epsilon)$. (ϵ is the frequency dependent complex dielectric constant of the material⁶.)

The ~ 15 eV loss found in all the bases is probably due to σ electron transitions, although energy losses in this region resulting from correlation effects of the π electrons have recently been discussed⁷.

We interpret the broad energy loss at ~ 20 eV as a collective excitation of the σ plus π electrons. This implies that the loss is characteristic of the molecules condensed in a solid film rather than only of the molecules themselves. In subsidiary measurements we found that the energy of the maximum of this peak had a strong dependence on the angle between the transferred momentum and the plane of the film, increasing to about 26 eV for a scattering angle of ~ 5 mrad (in this case the trans-

Table 1 Characteristic Energy Losses of Sublimed Films of the Common Nucleic Acid Bases

Base	<i>a</i>		<i>b</i>		<i>c</i>		<i>d</i>		<i>e</i>	<i>f</i>	<i>g</i>
	Energy loss (eV)	Ultra-violet (eV)	Energy loss (eV)	Ultra-violet (eV)	Energy loss (eV)	Ultra-violet (eV)	Energy loss (eV)	Ultra-violet (eV)	Energy loss (eV)	Energy loss (eV)	Energy loss (eV)
A	4.9 ± 0.1	4.56	6.3 ± 0.3	5.90	7.0 ± 0.2	6.80	8.0 ± 0.3	7.72	14.7 ± 0.5	20.5 ± 0.8	—
T	4.75 ± 0.10	4.62	5.9 ± 0.2	5.85	7.45 ± 0.10	7.04	8.7 ± 0.3	8.56	15 ± 1	21.2 ± 0.5	27.5 ± 1.0
U	4.85 ± 0.10	4.65	6.2 ± 0.2	6.05	7.4 ± 0.1	6.95	8.8 ± 0.3	8.56	14.8 ± 0.5	19.4 ± 0.5	26 ± 1
G	4.8 ± 0.2	4.40	5.3 ± 0.2	4.96	7.0 ± 0.2	6.20	8.8 ± 0.3	8.64	14.9 ± 0.5	20.2 ± 0.5	—
C	4.7 ± 0.2	4.57	6.5 ± 0.1	6.19	—	—	8.3 ± 0.2	8.24	14.0 ± 0.5	22.0 ± 0.5	—

Lower case letters at the head of each column refer to the spectra in Fig. 1. The energy loss values are those which we measured. Columns labelled ultraviolet refer to the vacuum ultraviolet measurements of Yamada and Fukutome³. All peaks indicated were clearly resolved in many spectra, with the exception of peaks *a* and *b* in guanine. They were resolved as separate peaks in only a few spectra. The positions of the energy loss peaks have not been corrected for the background.

ferred momentum was approximately parallel to the plane of the film⁸). We attribute this behaviour to a layered structure in the films giving an anisotropic behaviour similar to that of graphite⁸.

The ~25 V shoulders found in uracil and thymine may be due to the 23.7 eV L₁ level in oxygen⁹, for these are the only two bases studied which contain two oxygen atoms.

From the results of these preliminary experiments we conclude that it may be possible to use energy loss information to provide contrast in an electron microscope. Whether this potential can be realized will be determined by the rate at which energy loss information is obtained compared with the rate at which the molecules are damaged. These experiments do not yet give all the necessary information because the molecules are condensed in solid films. This results in the broad collective loss at ~20 eV, which not only affects the intensity of the more characteristic low energy transitions but also is the principal source of energy deposition in the specimen, and this in turn affects the radiation damage probability. To investigate this further, experiments are in progress with these and other biologically important molecules in different environments (for example, polynucleotides) and at low temperatures.

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A. V. CREWE
M. ISAACSON
D. JOHNSON

Enrico Fermi Institute
and Department of Physics,
University of Chicago,
Chicago, Illinois 60637

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Another Humoral Substance that enhances Adrenergic Responsiveness during Acute Renal Ischaemia

RENAL hypertension consists of an early humoral period in which angiotensin concentrations are increased¹ followed by a period of sustained hypertension. The latter seems to be maintained largely through the sympathetic nervous system²⁻⁴, but the mechanism is not fully understood⁵⁻⁷. The interaction between angiotensin and the sympathetic nervous system has received attention^{8,9}; that increased angiotensin-like activity has not been detected in blood^{10,11} and that renin and angiotensin concentrations are often not increased in chronic hypertension^{1,12,13} suggests that something other than the renin-angiotensin system accounts for the facilitation of neurogenically mediated vasoconstriction observed during sustained hypertension¹⁴⁻¹⁶. The neurogenic contribution to renal hypertension could develop if the ischaemic kidney produces another factor which acts like angiotensin or which potentiates the effects of undetectable amounts of angiotensin. Here we describe the release during acute reduction of renal perfusion pressure of a new humoral factor with actions like those of

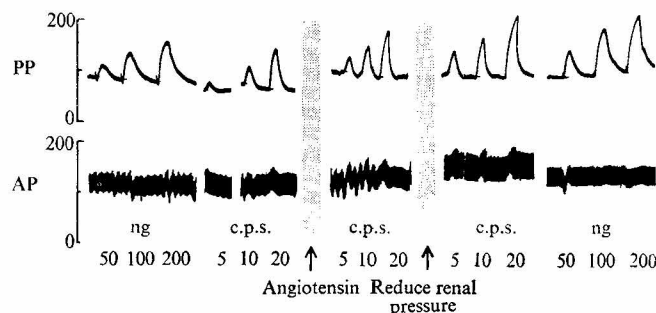


Fig. 1 Effect of reduced renal pressure on adrenergic responses during infusion of angiotensin. Hind paw perfusion pressure (PP) and systemic blood pressure (AP) are calibrated in mm Hg. The left hand portion shows responses to intra-arterial noradrenaline (50, 100, 200 ng) and sympathetic nerve stimulation (5, 10, 20 c.p.s.) before treatment. The middle segment shows responses to nerve stimulation repeated during infusion of angiotensin, at a dose of 0.4 µg/min, which had no further facilitating effect. The last segment shows those responses elicited 30 min after reduction of renal pressure and in the presence of the same angiotensin infusion.

angiotensin. The factor, released into renal venous blood, elicits hind paw adrenergic vasoconstrictor responses beyond the level produced by the infusion of angiotensin alone.

Adult mongrel dogs were anaesthetized with pentobarbitone sodium (30 mg/kg given intravenously) and supplementary doses were given as necessary. Tracheas were cannulated and the animals were respirated artificially with a Harvard respirator. Systemic blood pressure in a brachial artery was monitored by a Statham arterial pressure transducer. Heparin sodium (5 mg/kg) was used as an anticoagulant and was administered intravenously just before perfusion of the hind paw. The animals were prepared for constant flow perfusion of the hind paw using the method of Zimmerman⁸, the flow being kept constant by a Sigmamotor pump. The left kidney was exposed retroperitoneally by a flank incision and the renal vein was carefully dissected from its surrounding tissue. The left paw was perfused with renal venous blood obtained from either the ovarian or spermatic veins or from an extracorporeal circuit established from the renal vein to the jugular vein. The lumbar sympathetic chain was isolated and stimulated at supramaximal voltage with 2 ms pulses for 15 s with a Harvard electrode. The renal pressure was reduced by tightening a Blalock clamp placed on the aorta above both renal arteries. The deep circumflex artery was cannulated; it served to measure arterial pressure below the kidney. Pressure to the kidneys was reduced by 50%. Noradrenaline (50, 100, 200 ng) was injected intra-arterially into the tubing circuit as it entered the paw. Angiotensin ('Hypertensin', Ciba) and prostaglandin E₂ (1 µg/min) were infused (0.1 ml/min) into the tubing circuit as it entered the paw using a Harvard infusion-withdrawal pump. Data were analysed statistically by the Student's *t* test for paired comparisons (one tailed)¹⁷.

To eliminate the effects of angiotensin released during reduced renal pressure, adrenergic responses were studied in the dog hind paw perfused with renal venous effluent in which vasoconstrictor responses to angiotensin were already at a maximum. In each experiment an amount of angiotensin which produced a facilitation of the vasoconstrictor responses to sympathetic nerve stimulation was infused into the paw. The dose of angiotensin was then increased until no further facilitation was observed. The assumption made was that any further increase in the response to sympathetic nerve stimulation observed during renal ischaemia must have been due to another humoral factor.

A typical experiment is shown in Fig. 1. Control responses to intra-arterial noradrenaline and sympathetic nerve stimulation are shown first. Next, angiotensin was infused into the paw to increase the response to nerve stimulation. In this experiment, at 0.4 µg/min, no further increase in the response to nerve

stimulation was observed. The remainder of Fig. 1 illustrates that when this amount of angiotensin was continued during the period of reduced renal pressure, the responses to both intra-arterial noradrenaline and sympathetic nerve stimulation were augmented. It can also be seen that arterial pressure rose.

Figs. 2 and 3 summarize the effects of vasoconstrictor responses produced by nerve stimulation and noradrenaline. In both cases, responses were significantly increased when renal pressure was reduced during the period of maximum facilitation of vasoconstriction by angiotensin. In other experiments, no facilitation of vasoconstrictor responses beyond that obtained by angiotensin was observed when the angiotensin infusion was continued without reduction of renal pressure. Thus time alone was not responsible for the augmented responses seen during the period of reduced pressure.

Recent evidence^{18,19} suggests that prostaglandin-like substances are released from the ischaemic kidney. In three experiments, animals were studied as described, except that prostaglandin E₂ (1 µg/min) was infused into the paw after vasoconstrictor responses were enhanced by angiotensin. A further facilitation to sympathetic nerve stimulation was observed during prostaglandin infusion. The mean increases in perfusion pressure during angiotensin infusion were 48, 85 and 119 mm Hg for 5, 10, 20 c.p.s. respectively. When prostaglandin was infused, the mean values increased to 99, 137 and 175 mm Hg. Responses to noradrenaline were unchanged.

We have sought evidence for the elaboration of a humoral factor, in addition to angiotensin, which could enhance the ability of angiotensin to facilitate adrenergic vasoconstriction or which could itself facilitate adrenergic transmission. Adrenergic responses were studied during infusions of angiotensin in excess of the amount required to cause maximum facilitation of vasoconstriction produced by nerve stimulation. Any further increase in the vasoconstrictor response to sympathetic nerve stimulation during renal ischaemia must therefore have been due to the elaboration of something other than angiotensin.

The results of this study indicate that the kidney apparently elaborates an additional humoral factor which facilitates the response to sympathetic nerve stimulation above that attributed to the facilitation observed with angiotensin alone. The re-

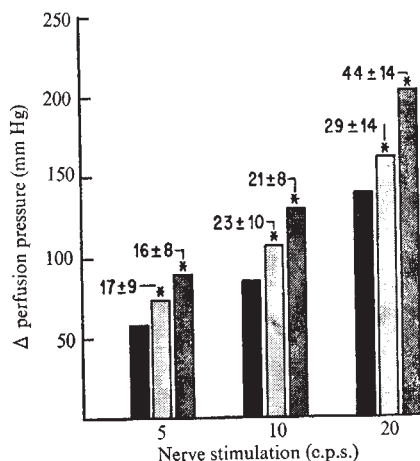


Fig. 2 Facilitation of the responses to sympathetic nerve stimulation by angiotensin and reduced renal pressure. The ordinate represents the change in perfusion pressure induced by stimulation of the lumbar sympathetic chain at 5, 10, 20 c.p.s. The black bars represent control responses. The light shaded bars represent responses obtained during infusion of amounts of angiotensin greater than required to cause maximal facilitation of vasoconstriction produced by nerve stimulation. The dark shaded bars are responses obtained when renal pressure was reduced during angiotensin infusion. An asterisk above the light shaded bars denotes a significant difference from control ($P < 0.05$). An asterisk above the dark shaded bars represents a significant difference from angiotensin. Values above the bars are mean differences $\pm$ s.e. for paired comparisons. $n = 15$.

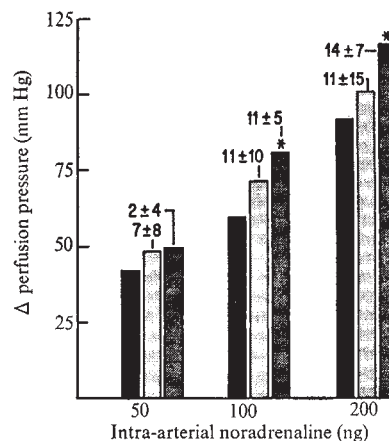


Fig. 3 Facilitation of the responses to intra-arterial noradrenaline by angiotensin and reduced renal pressure. The ordinate represents the change in perfusion pressure as a result of vasoconstriction induced by intra-arterial noradrenaline (50, 100, 200 ng). The black bars represent control responses. The light shaded bars represent responses obtained during infusions of amounts of angiotensin greater than required to cause maximal facilitation of vasoconstriction produced by nerve stimulation. The dark shaded bars represent responses during reduced renal pressure. Values shown are the mean difference $\pm$ s.e. An asterisk above the light shaded bars denotes a significant difference from control ($P < 0.05$). An asterisk above the dark shaded bar represents a significant difference from angiotensin. $n = 15$.

sponses to exogenous noradrenaline were also augmented during renal ischaemia suggesting that increased sensitivity at the receptor level could play a role in enhancement of neurogenically mediated responses. The precise mechanism of the facilitation and the nature of the factor are not known. Preliminary experiments suggest that the factor could be a prostaglandin because several of these agents produce effects on adrenergic transmission^{20,21} similar to those of angiotensin and because the responses to sympathetic nerve stimulation were facilitated by prostaglandin E₂ in the presence of angiotensin.

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CHARLES S. SWEET
PHILIP J. KADOWITZ
MICHAEL J. BRODY

Department of Pharmacology,
School of Medicine,
University of Iowa,
Iowa 52240

Received November 2, 1970.

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Tumour Immunity induced in Mice with Cell-free Homogenates of Influenza Virus-infected Tumour Cells

THE concept of reinforcing the immunological rejection reaction to a tumour by immunizing the host with tumour cells which have been modified by the introduction of a "helper determinant" onto their surfaces has been reviewed recently¹. In general, tumour specific transplantation antigens (TSTA) on the surface membranes of tumour cells stimulate a relatively weak rejection reaction. If a strongly immunogenic determinant is coupled to the tumour cells, and the cells are then inoculated back into the host, a more intense immune response may be elicited which is also active against the TSTA alone. The helper determinant can be a hapten, a protein, a histocompatibility antigen (introduced into the tumour cell by hybridization with allogeneic cells), or a myxovirus which matures at the tumour cell surface. An early example of the use of a myxovirus as helper determinant is the work of Lindenmann and Klein², who showed that A2G mice could be made immune to challenge with Ehrlich ascites tumour cells by vaccinating them with a cell-free homogenate of tumour cells which had been infected with influenza virus. Vaccination with a mechanical homogenate of uninfected tumour cells was without protective effect. Axler and Girardi³ prevented the expected appearance of tumours in hamsters due to inoculation of newborns with SV40 virus by vaccinating them during the latent period with homogenates of Newcastle disease virus-infected, SV40-transformed hamster tumour cells. Frozen-thawed lysates of non-NDV-infected SV40-transformed cells had no effect. We report here that BALB/c mice can be made immune to challenge with syngeneic, SV40-transformed fibrosarcoma cells by vaccinating them with cell-free homogenates of tumour cells which have been infected with influenza virus. No immunity is induced by vaccinating the mice with homogenates of influenza virus-infected normal cells, or with homogenates of tumour cells not previously infected with influenza virus.

The syngeneic tissue cultured tumour cell line used—SV3T3-T4—was obtained as an explant from a solid fibrosarcoma in a BALB/c mouse inoculated with SV40-transformed BALB 3T3 cells⁴. Two lines of SV3T3-T4 cells were expanded through approximately eight subculture generations and stored viable-frozen in tissue culture medium containing 7.5% dimethyl sulfoxide: one was grown in 10% horse serum and subcultured by scraping; the other was grown in 10% foetal bovine serum and subcultured by trypsinization. The SV3T3-T4 and BALB 3T3 cells used to make cell-free homogenates for immunization were grown in foetal bovine serum. Thus, the same foreign serum or trypsin proteins were never present in both vaccinating and challenge inocula. A subcutaneous dose of 10^6 SV3T3-T4 cells produced tumours which grew to a mean diameter of 1 cm within 30 days in 80–100% of mice. The tumours invariably grew progressively and killed the animals.

We used the WSA strain of influenza A₀ virus (supplied by

J. Lindenmann) which had been adapted by Ackerman and Kurtz to grow in Ehrlich ascites tumour carried in ICR mice. The strain was passed in suckling BALB/c mice by intracerebral inoculation, then adapted to the SV3T3-T4 and BALB 3T3 lines. The virus produced marked cytopathic effects after three passages and high haemagglutinin titres after seven passages.

The immunization procedure consisted of inoculating 0.5 ml. of diluted homogenate intraperitoneally on day 0 and 0.25 ml. intraperitoneally and subcutaneously on day 3. After 5–7 days, the mice inoculated with virus-infected homogenates became stuporous, about 10% of them died, and about 5% of the rest developed paralysis of the lower extremities. (Preliminary results show that formaldehyde-inactivated homogenates do not kill or paralyse animals and also produce tumour immunity.) On day 14, the mice were challenged subcutaneously with 10^6 SV3T3-T4 cells. One month after challenge, all subcutaneous tumours which appeared were dissected out and weighed.

Fig. 1A shows that almost complete protection against tumour challenge was induced by vaccinating the mice with 10^{-1} and 10^{-2} dilutions, and part protection with a 10^{-3} dilution, of homogenates of SV3T3-T4 cells which had been infected with influenza virus. By contrast, homogenates of the same tumour cells which had not been infected with influenza virus did not induce immunity (Fig. 1B). Fig. 1C shows that the tumour immunity associated with influenza virus infection is specific for the SV40-transformed tumour cells: vaccination with influenza virus-infected normal 3T3 cells did not induce immunity to challenge with SV3T3-T4 cells. Tumour growth in unvaccinated mice was the same as that in mice inoculated with the tumour homogenates which had not been infected with influenza virus (Fig. 1D).

We determined that 5×10^6 X-irradiated SV3T3-T4 cells inoculated subcutaneously into mice on days 0, 3 and 7 would induce complete immunity to the usual challenge dose of 10^6 SV3T3-T4 cells on day 14. Inoculation of normal 3T3 cells in the same manner did not induce such immunity. Viable SV3T3-T4 cells therefore possess an SV40-induced TSTA on their surface which is rendered undetectable if the tumour cells are homogenized. As we have shown, the immunogenicity of the TSTA is retained in homogenates of tumour cells provided that they have been previously infected with influenza virus.

Certain aspects of the replicative behaviour of influenza virus in the host cell are clearly related to the mechanism by which it enhances tumour immunity. As the virus matures and buds from the host cells, the entire plasma membrane of the cell, not just the portion associated with budding virus, becomes antigenically modified because of the insertion of virus-coded antigenic proteins (V antigen) into its structure⁵. When influenza virus-infected cells are disrupted, plasma membrane microvesicles possessing haemagglutinating and V antigenic proteins in their surface structure are produced. These host cell membrane-derived structures, which absorb to and elute from erythrocytes and induce neutralizing and haemagglutinin-inhibiting antibodies, have been called "incomplete virus", although they are not precursors of the virus⁵. Duc-Nguyen *et al.*⁵ have clearly shown by ferritin labelling that normal host membrane antigens are not incorporated into the membranous envelope of the virus as it buds from the cell, but that host membrane antigens and viral antigens do occur together in the plasma membrane microvesicles from disrupted cells. In the case of the influenza virus-infected SV3T3-T4 cells therefore the immunogenic unit is likely to be a plasma membrane microvesicle containing two determinants foreign to the host: the SV40 TSTA and the influenza virus V antigen.

If the influenza virus-mediated enhancement of tumour immunity presented here were to operate through humoral immune mechanisms, current theory⁶ would stipulate that the two foreign determinants, SV40 TSTA and influenza virus V

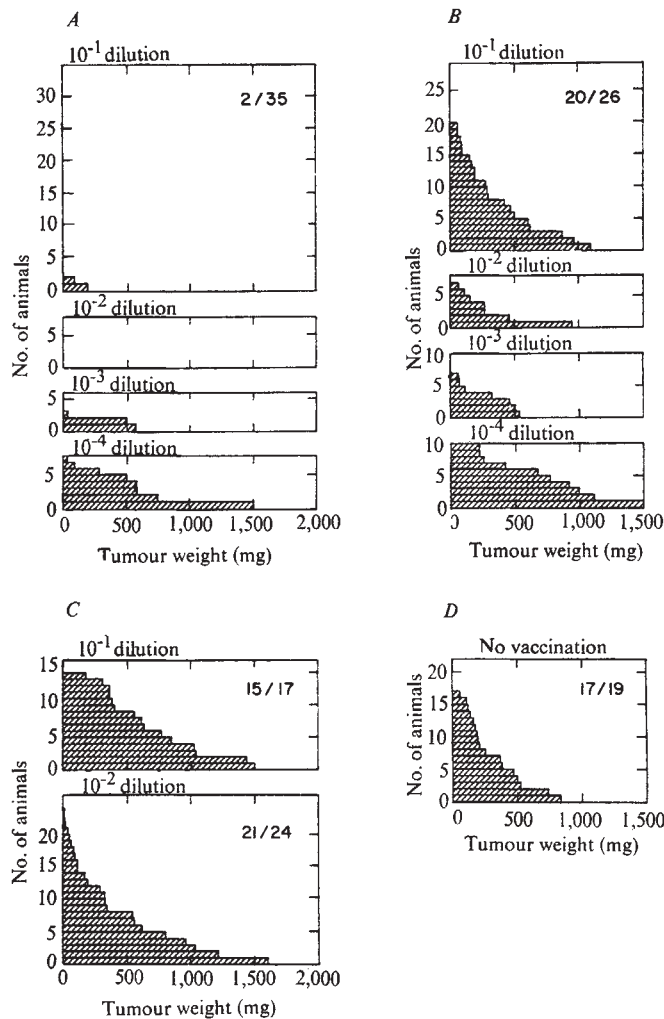


Fig. 1 Tumour immunity induced by vaccination with cell-free homogenates of influenza virus-infected tumour cells. All mice were challenged with 10⁶ tumour cells 14 days after the start of vaccination with cell-free homogenates at the various dilutions shown. *A*, Vaccinated with a homogenate of tumour cells previously infected with influenza virus; *B*, vaccinated with a homogenate of tumour cells which had not been previously infected with influenza virus; *C*, vaccinated with a homogenate of normal cells previously infected with influenza virus; *D*, not vaccinated. Homogenates of influenza virus-infected cells were prepared as follows. Approximately 300 million monolayer cultured cells were inoculated with 10² haemagglutinin units of virus and harvested 3 days later. A cell-free homogenate was prepared by rapidly freezing and thawing three times 1 ml. of packed infected cells in 10 ml. of culture medium (10⁻¹ dilution). The final haemagglutinin titre of the homogenate was about 10³ haemagglutinin units per ml. and the LD₅₀ titre of both SV3T3-T4 and BALB 3T3 homogenates inoculated intracerebrally in weanling mice was 10⁻⁸ ml. Homogenates of uninfected SV3T3-T4 cells were prepared by rapid freezing and thawing of the cultured cell monolayers three times, followed by shaking of the remaining cells off the bottom of the cultured vessels. The final homogenate was of 1 ml. of packed cells in 10 ml. of culture medium (10⁻¹ dilution).

antigen, present together on the insoluble plasma membrane microvesicle, constitute a strong immunogen; without the V antigen "helper", the SV40 TSTA determinant alone is less immunogenic and may be toleragenic. Whether such a "helper" mechanism applies also to the cellular immune response, and, in particular to the case of influenza virus-mediated tumour immunity, remains to be determined.

An obvious possible future use of the influenza virus effect is in the immunotherapy of human tumours. Primary tumour tissue which is removed from the patient, infected with influenza virus, homogenized, inactivated with regard to infectivity, and then inoculated back into the patient, could conceivably

enhance the patient's immune resistance to residual tumour and to the development of metastases.

C. BOONE
K. BLACKMAN
P. BRANDCHAFT

Cell Biology Section,
Viral Biology Branch,
National Cancer Institute,
National Institutes of Health,
Bethesda,
Maryland 20014

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Detection of Australia-SH-Antigen in Urine

ACCUMULATING evidence suggests an aetiological relationship between Australia (Au/SH)-antigen and serum hepatitis¹⁻⁵. So far this antigen has been demonstrated in the serum of diseased and apparently healthy persons only. It has been shown that serum hepatitis can be transmitted orally in humans^{6,7}, so that the demonstration of this antigen in excretions (urine, faeces, sputum and so on) might be of great epidemiological interest. Because the antigen is likely to be highly diluted in these materials, we concentrated the urine by affinity adsorption chromatography^{8,9}. About 500 ml. of urine adjusted to pH 7.2 was put through a column of 'Sephacrose 4 B' (1.0 × 5.0 cm), which was charged with an Au/SH-antigen antiserum (20 mg

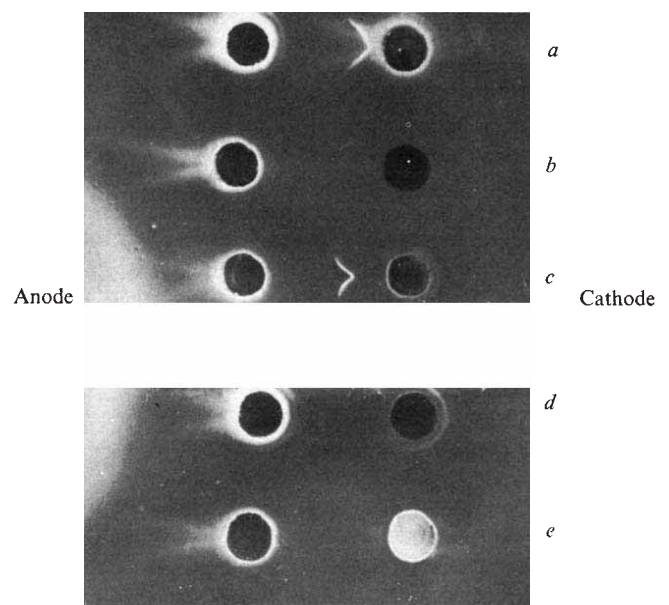


Fig. 1 Immuno-electrophoretic demonstration of Au/SH antigen in urine of a patient with serum hepatitis. The left well contained Au/SH antiserum; the right well contained: *a*, serum of a patient (precipitation); *b*, native urine of same patient (no precipitation); *c*, urine of same patient after concentration (precipitation); *d*, serum of a healthy person (no precipitation); *e*, serum of same healthy person after concentration (no precipitation).

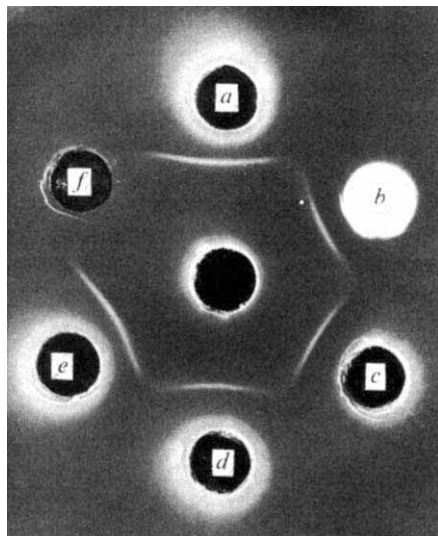


Fig. 2 Demonstration of immunological identity between Au/SH antigen concentrated from urine and serum of a patient with hepatitis and a control antigen, applying the two dimensional micro-Ouchterlony immunodiffusion test. The central well contained Au/SH antiserum; a, d and e, control antigen; f, control antigen in low concentration; b, concentrated urine; c, serum of same patient.

of protein/ml. of 'Sephacrose', flow rate, 50 ml./h). After the column had been washed with about 50 ml. of 0.15 M phosphate buffer, pH 7.2, the bound antigen was eluted by using 0.15 M glycine-HCl buffer, pH 1.8. The eluate of about 10 ml. was further concentrated by vacuum ultrafiltration to a final volume of about 0.2 ml., and subjected to immunoelectrophoresis¹⁰. We had checked the sensitivity of the method by diluting 0.1 ml. of serum containing Australia antigen with 500 ml. of water, and confirmed that we could isolate the antigen again. We routinely test sera of patients with suspected liver disease for the presence of Au/SH antigen by immunoelectrophoresis, and the urine of positive cases is further investigated according to the method we have described.

Fig. 1 illustrates the results obtained from a 84 year old female patient with icteric hepatitis compared with those from a healthy person. There was Au/SH antigen in both the serum and the urine of the patient. The two dimensional micro-Ouchterlony immunodiffusion test showed that the antigen in the serum was immunologically identical to that in the urine (Fig. 2). As far as we know, this is the first time Au/SH antigen has been demonstrated in urine^{11,12}. The excretion of Au/SH antigen may be a potential hazard for infection, especially if these materials are not handled carefully. Thus the high rate of hepatitis among personnel in, for example, dialysis units may be explained¹³. We are in the process of investigating other possible sources of contamination (for example faeces, sputum, saliva, syringes, needles, sewage and swimming-pool water. Furthermore, blood sucking insects such as mosquitoes and fleas may act as vectors of serum hepatitis.

IOANNIS TRIPATZIS
HANS G. HORST

*Institut für Hygiene und Med. Mikrobiologie
der Medizinischen Akademie,
24 Lübeck 1, Ratzeburger Allee 160*

Received July 7, 1970.

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Electron Microscopic Studies of Lipid Protein Films

PROTEINS can be adsorbed from the solution on lipid monolayers at the air water interface. One may expect that a lipid protein film is formed consisting of a closely packed layer of protein molecules attached to the hydrophilic side of the lipid film. The quantity of protein adsorbed can be evaluated by measuring the optical extinction, but this value is only an average of the spatial distribution of the protein molecules¹. We have obtained information on the molecular arrangement by electron microscopy.

Ferritin molecules (horse spleen ferritin, Fluka AG Buchs) adsorbed on a lipid monolayer form a two dimensional closely packed lattice (Fig. 1). The lipid protein film is formed by placing the lipid film (stearic acid methyl ester and eicosic trimethyl ammonium bromide (4 : 1), surface pressure 30 dyne/cm) on a ferritin solution (concentration 20 g/l., veronal buffer pH 5) by means of a multicompartiment trough¹. After an adsorption time of 15 min the lipid protein film separated from the protein solution and transferred to a glutardialdehyde solution for fixation. It is then placed on a glass slide covered by a layer of anodically oxidized aluminium and some cadmium arachidate monolayers. The alumina film together with the lipid layers is detached from the slide with HgCl₂ (refs. 2 and 3) and deposited on a copper grid. The ferritin molecules observed in the electron microscope have a regular arrangement (Fig. 1), either because the rate of formation of "seeds" is slow and the two dimensional crystallites grow, starting from a few nuclei, or because the ferritin molecules are adsorbed at random and order themselves afterwards. It is not yet known which of these mechanisms operates. Other arrangements are observed with other lipid monolayers.

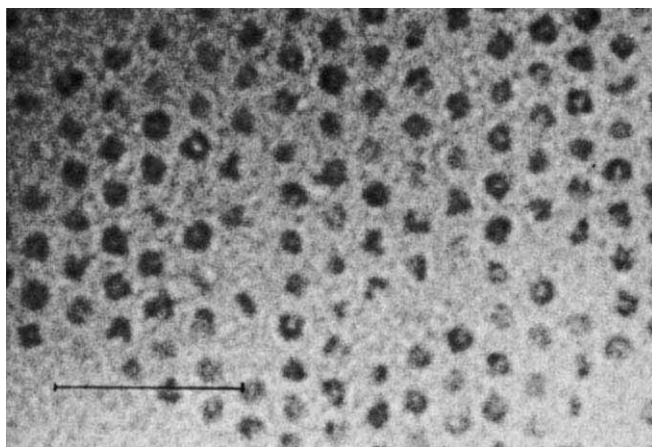


Fig. 1 Electron micrograph of ferritin molecules adsorbed on a lipid monolayer composed of stearic acid methyl ester and eicosic trimethyl ammonium bromide (4 : 1), and fixed with glutardialdehyde. The diameter of the dark spots is about 75 Å which agrees with the value for the iron-containing core of ferritin found by earlier electron microscopic and X-ray investigations^{4,5}. The centre to centre distance is about 137 Å which is thus somewhat greater than the diameter of a ferritin molecule (122 Å)^{4,5}. (The electron microscope was a Philips EM 300; the calibration bar equals 500 Å.)

I thank Dr K. H. Langer for taking the electron micrographs and Professor H. Kuhn for discussions.

PETER FROMHERZ

Physikalisch-Chemisches Institut der
Universität Marburg, and
Max-Planck-Institut für Biophysikalische Chemie,
Göttingen

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Attraction of Triatomine Bug Vectors of Chagas's Disease to Betalights

APART from the improvement of housing standards as circumstances permit, the control of Chagas's disease throughout its range in South and Central America depends on control of the vectors (domestic triatomine bugs) by periodic indoor application of residual insecticide, usually HCH. Bug infestation in the low standard dwellings which are characteristic of urban and rural foci of the disease is usually discovered by daytime search for bugs in the cracks and crevices of walls and other indoor surfaces where the insects hide during the day. Pyrethrum powder is often blown into such fissures to irritate the bugs and cause them to emerge¹, and some workers also sample by attaching perforated boxes containing crumpled paper to walls, when the chance lodgment of bugs in the boxes may indicate infestation². The economic management of vector control requires more reliable and less time-consuming sampling methods³ and there is also a need for better quantitative data on bug densities in relation to clinical and epidemiological aspects of the disease.

It is therefore relevant that light attracts these nocturnal bugs. Sylvatic species, commonly living in palms and other arboreal environments, sometimes enter lighted dwellings⁴ or are attracted to lighted roadways⁵ and to ultraviolet light in outdoor traps^{6,7}. Some preliminary laboratory investigations on the attraction of *Triatoma maculata* to betalights are described here. Betalights, discussed elsewhere as attractants in mosquito larval traps⁸, give out no heat and glow continuously for years without attention or the need for electrical or any other extraneous source of power. They are available in different shapes, colours and sizes, and the findings described here suggest that they may be useful attractants in traps for triatomine bugs.

Bugs were released into an opaque white polyethylene sandwich box (30.5 × 20.3 × 7.6 cm) with, at each end on the mid-line, a semitransparent polyethylene spray bottle (7.6 cm × 4.5 cm in diameter) screwed, after discarding the stopper, into a hole cut about 0.5 cm above the floor of the chamber. One bottle—the betalight trap—had a spherical (10.9 mm diameter) green betalight of 2,000 microlamberts luminance (S10/G/2000—Saunders-Roe Developments Ltd) fixed with adhesive tape to a hole in the base of the bottle. The control trap, similarly taped, was not illuminated. Damp wool slung from the chamber lid maintained humidity and filter paper on the chamber floor facilitated movements by the bugs. The apparatus was continuously in the dark at 24°–25° C except for a few seconds' exposure to light at half hourly, hourly or other intervals, when observations were made of the distribution of the bugs in the chamber and traps.

Fig. 1 summarizes, in 3 h periods, results for ten unfed male *T. maculata* released into the chamber. Their distribution in the apparatus, based on hourly observations, was recorded for 34 h consecutively. At each hourly observation any bugs in the traps were returned to the chamber. Thus, thirty bugs

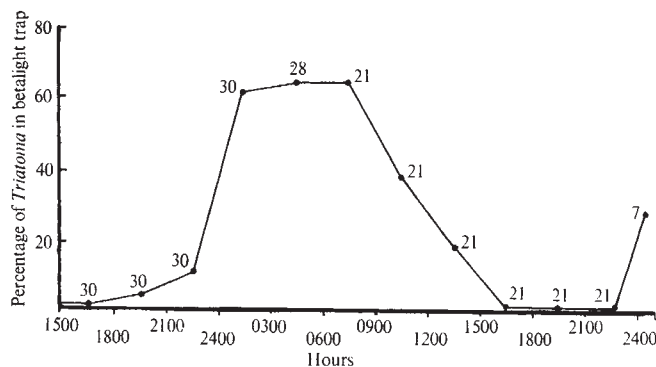


Fig. 1 Percentage of male *Triatoma maculata* entering in successive 3 h periods a green betalight trap (S10/G/2000) during a 34 h experiment carried out in darkness except for brief inspections, at hourly intervals, to record the distribution of the bugs in the central chamber, control trap and betalight trap. The number at each point is the number of bugs available in each period to be attracted to the trap. There seems to be a diel rhythm of attraction, mainly during night hours, to the betalight.

were available to be attracted in the first four 3 h periods but, owing to deaths, twenty-eight and twenty-one later, and only seven for the final hour.

Bugs were never found in the lightless control trap. In several observations between 0900 h and 1500 h, preceding the graphed data of Fig. 1, none entered the betalight trap either, although the apparatus was in darkness and the betalight glowed continuously. This inactivity continued, except in the case of a single bug in the betalight trap between 1900 h and 2000 h, until between 2100 h and 2400 h when 9.9% of the bugs entered the betalight trap. Attraction to this trap then rose rapidly to 61% and persisted about this level for most of the night before declining steadily to zero by 1600 h on the following day. Attraction was not apparent again until the final hour of observation, 2400–0100 h. In another experiment, there was no catch of male *T. maculata* in either trap from 1200 h to 2130 h followed by catches, only in the betalight trap, from 2200 h to 0930 h, with a maximum catch between 0230 h and 0300 h. A diel rhythm of attraction to betalights, particularly during night hours, appears to be involved. In other experiments, weaker betalights in red (40 microlamberts), blue (90 microlamberts) and white (200 microlamberts) attracted male, female, and late larvae of *T. maculata* in 50–70% of trials. Further investigations of betalights as convenient attractants in traps for triatomine bugs are in progress.

I thank numerous entomologists and doctors in Chile, Argentina, Brazil, Venezuela and California for information about triatomine problems during a visit with Professor W. H. R. Lumsden of this school to these countries in 1970, and the Wellcome Trust for finance under the Wellcome/London/Harvard scheme for overseas research. R. A. and K. L. Bertram and Dr D. M. Minter helped with some of the observations.

D. S. BERTRAM

Department of Entomology,
London School of Hygiene and Tropical Medicine,
Keppel Street, Gower Street, London WC1E 7HT

Received January 6, 1971.

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BOOK REVIEWS

Clash of Ideas

Criticism and the Growth of Knowledge. Edited by Imre Lakatos and Alan Musgrave. (Proceedings of the International Colloquium in the Philosophy of Science, London, September 1970.) £3.50 boards; £1.00 paper.

ACCORDING to Popper, the best hope of achieving progress in science is to put forward bold, imaginative speculations, controlled by rigorous criticism, by a ruthless search for experimental refutations. Kuhn, however, thinks differently. According to Kuhn, a science begins to make real progress just when its practitioners forgo criticism of fundamentals, and instead elect to extend and elaborate some basic theory or paradigm, which may be accepted even in the face of known anomalies or "refutations". According to Kuhn, it is just this type of "normal" scientific research which best leads to the discovery of serious objections to the accepted theory, and thus to the eventual development of some revolutionary new theory. It is the clash between these two viewpoints that is explored in the nine essays of this book. On the whole this is an interesting and valuable collection of papers.

Kuhn's first contribution is a criticism of Popper's views on scientific method. Kuhn has some quite valuable things to say. Unfortunately he presents the difference between Popper's views and his own as a difference of emphasis only, as a *gestalt* phenomenon of seeing a different pattern in the same drawing. This mode of presentation tends to under-emphasize the substantial issues which divide Popper and Kuhn. Watkins and Popper contribute clear but somewhat rigid critiques of Kuhn's position from the standpoint of Popperian orthodoxy. Both deplore normal science as hack science, and yet do not really stop to consider Kuhn's best arguments in support of the methodological importance of normal science. Margaret Masterman examines the multitudinous interpretations that may be given to Kuhn's key notion of a "paradigm". Toulmin argues, in my view wholly unconvincingly, that if scientific revolutions occur too often they become a part of "normal" science, and Kuhn's distinction between the two collapses. In a two-page contribution, Pearce Williams argues that the only way to decide between Kuhn and Popper is to appeal to the history of science, but that as yet not enough is known to decide the

issue. This cannot be quite right because the issue between Kuhn and Popper is not simply how science does proceed, but rather how science ought to proceed. This last point is taken up by Feyerabend, who criticizes Kuhn for not making it clear whether his account of scientific enquiry is descriptive or prescriptive. (To this Kuhn replies, a little unsatisfactorily, that it is both.) Taking it to be the latter, Feyerabend considers Kuhn's thesis that normal science is desirable in that it tends to precipitate revolutions, and argues that Kuhn is not really entitled to hold this thesis in that he can give no reason why revolutions are themselves desirable. Feyerabend goes on to criticize the Popperians for not making good their claim to exhibit science as a rational enterprise, and makes a plea for hedonism in science.

Lakatos, in a long and extremely interesting paper, argues that we should see science as a struggle between competing research programmes, competing fragments of Kuhn's normal science, each programme being based on some "core" theory or paradigm. No research programme can be decisively eliminated. Rational choice remains a possibility, however, for it is rational to choose that programme which grows the fastest, which successfully predicts the greatest range of new phenomena. Lakatos thus sets out to reconcile Kuhn's normal science with Popper's permanently revolutionary science.

In his final contribution Kuhn says that "In all but a few respects, only two of them essential . . . [Lakatos'] position is now very close to mine". A kind of uneasy resolution is then achieved on some of the issues involved. On one issue, however, there is scarcely even communication, let alone a resolution. Again and again the Popperians accuse Kuhn of portraying science in psychological or sociological terms only, theory-choice in particular being only a question of mob rule, rational arguments having no part to play. Kuhn rejects the allegation in amazement.

A part of the explanation for this impasse is, I think, that the disputants, without altogether realizing it, mean quite different things when they passionately affirm that science is rational. For Kuhn, this seems to mean simply that science proceeds in accordance with certain general methodological rules. For the Popperians, it seems to mean in addition that we have some reason for holding that the methods of science give us the best hope of pro-

gressing towards the truth, or at least of detecting error in our theories. Not only do the Popperians fail completely to solve the problem of providing this second kind of rationale for the methods of science, however; they scarcely seem to be aware that there is a problem at all. Lakatos, for example, cheerfully admits that the methodological rules he advocates lack any kind of rationale. Kuhn, who is aware of this problem, and thinks it insoluble, naturally becomes somewhat impatient when he is castigated for failing to meet standards of rationality which his critics, did they but know it, do not meet either. NICHOLAS MAXWELL

Outlines of Energy Flow

Environment, Power and Society. By Howard T. Odum. Pp. ix+316. (Wiley-Interscience: New York and London, February 1971.) £4.50 boards; £2.65 paper.

A FAMILIAR aspect of the interaction between man and his environment is the fact that eventually there will be no more fossil fuels left to subsidize man's activities. Less obvious is the importance of the energy-flows between and within the countless ecological subsystems of the biosphere. The energy-flows of a complex natural system can be represented diagrammatically in terms of a small set of basic components, each of which symbolizes some function common to all such systems—for example, energy storages and work-control gates.

The book opens with an outline of the world system, and in the second chapter the energy-flow diagram technique is developed. Subsequent chapters contain detailed discussion of the nature of energy-flows in various ecosystems, with emphasis on the effects of man's increasing dependence on fossil fuels. A close study is made, in energy-flow language, of information and money flows and of the stabilization and control effects arising from political and religious institutions. The most exciting prospect engendered by the use of energy-flow diagrams is the possibility of the simulation of energy networks by passive and active electrical analogue techniques. The closing chapters unfold the invaluable planning facilities afforded by such techniques, and put forward some speculations as to the nature of life in a world so planned.

The use of diagrams depicting the

flow of materials, minerals, money, energy, or whatever, is not new, as the author himself points out. What has been achieved in this book is the unification of all these instances into a single programme with a common measurable factor—the flow of energy. Thus we have a facility for studying old problems from new angles, and a source of sound arguments upon which to base necessary changes in many of man's habits. Professor Odum's book contains a highly readable introduction to the problems and methods of ecological studies, including a considerable bibliography of established and current research literature. Moreover, at a time of mounting concern over the future of man's partnership with nature, it provides the essence of what may be an ideal tool for a detailed qualitative and quantitative survey of that partnership.

D. I. McLAREN

Birds of Africa

African Birds of Prey. By Leslie Brown. Pp. 320+12 plates. (Collins: London, February 1971.) £2.25.

A Field Guide to the Birds of Southern Africa. By O. P. M. Prozesky. Pp. 350+40 plates. (Collins: London, January 1971.) £2.50.

LESLIE BROWN, a zoologist by training, has lived for many years in Africa and has made a special study of its birds of prey; he is also joint author of the standard work on the birds of prey of the world. With this authority he has now distilled some of his knowledge in an interesting and very readable book. In three introductory chapters he discusses the definition of a bird of prey, the behaviour and physical adaptations for predation, and the variety of birds of prey found in Africa—this last particularly in relation to the different types of habitat.

The next dozen chapters review, group by group (sometimes more by function than by strict taxonomy), the estimated 89 species of diurnal and 31 species of nocturnal raptors found in continental Africa as a whole. This takes us all the way from cuckoo-falcons to fishing-owls, with such chapter headings as "Large and spectacular eagles" and "A variety of middle-sized owls". There is a group entitled "Oddities" which contains the osprey, the vulturine fish-eagle (palm-nut vulture), the harrier-hawk, and—oddest of all—the secretary-bird. Without the tedium of a stereotyped treatment, suitable only for a work of reference, an account is given of the characteristics, distribution and habits of each species; personal experience of the birds is constantly cited.

After laying this foundation, the

author proceeds to the most important part of the book, a further nine chapters dealing with various biological problems that birds of prey present or exemplify. These are problems of ecology and distribution, of migration into and within Africa, of population dynamics and the effects of predation on other animals, of breeding seasons and cycles, and of relations with mankind. These matters are discussed in an interesting and often stimulating way. The book concludes with a valuable series of tables lucidly setting forth information about the abundance, distribution, habitats and food of each species.

Mr Prozesky's book provides a convenient conspectus of the nearly 900 bird species found south of the Zambezi. The text, desirably, follows the Peterson pattern which has become conventional for field-guides. The illustrations, of more than 400 species, fall substantially below the admittedly exacting Peterson level of artistry, but can for the most part be accepted as adequate for identification purposes.

A. LANDSBOROUGH THOMSON

Radiocarbon Dating

Radiocarbon Variations and Absolute Chronology. Edited by Ingrid U. Olsson. (Proceedings of the Twelfth Nobel Symposium held at the Institute of Physics at Uppsala University. Nobel Symposium No. 12.) Pp. 652. (Almqvist and Wiksell: Stockholm; Wiley-Interscience: New York and London, 1970.)

SINCE radiocarbon dating was first developed there has been a continued interest in the absolute accuracy of the method, and much effort has been expended, particularly during the past decade, in comparing radiocarbon with other systems of dating. The literature of the subject prior to this publication reveals a complex picture not easily understood by many users of radiocarbon dates and likely, in view of its complexity and the reported wide divergencies between radiocarbon and calendar dates in certain regions of time, to raise considerable doubts particularly in the mind of the archaeologist as to the validity of radiocarbon dates in general.

I would like to be able to report that this publication clarifies these problems completely for the general reader and user of radiocarbon dates but unfortunately this is not so. Despite this, however, it is the most comprehensive and up-to-date treatment of the subject at present available, and consequently all who are at all involved in using or producing radiocarbon dates will wish to acquire it or at least to ensure access to it.

It brings together under one cover

contributions from major workers in this field and there is much new work reported and much of great interest. Topics covered include the latest developments in those dating techniques which provide a means of comparison with the radiocarbon method: varve chronology, dendrochronology, pottery dating by thermoluminescence, ice core analysis, and of course the use of archaeological material of known age. In addition, there is equally full treatment of the many factors connected with the causes of the secular variations in radiocarbon activity including exchange rates in the various exchange reservoirs, the constancy of cosmic radiation as deduced from radiogenic isotopes in meteorites and on Earth and the effects of geomagnetic and heliomagnetic and climatic changes in the past.

Apart from the formal papers, the discussions are also recorded here although in condensed form. In places they are very illuminating. Thus, the editor states in the preface that she had "intended to publish most of the measurements on the radiocarbon content of the atmosphere in one figure . . . in such a way that this figure could be used for calibration purposes . . .". The reason why this was not possible emerges very clearly in the discussions. We learn, for example, that while there is fairly general agreement concerning the major long-term variations over the past seven millennia as deduced from measurements on dendrochronological material, the various laboratories are not in complete agreement as to the position and magnitude of the short-term fluctuations, and that while the majority were willing to pool their measurements in order to facilitate the editor's purpose, there was one notable exception (Professor Suess) who insisted that his calibration curve be published separately. Thus the chief shortcoming of the book from the point of view of the user of radiocarbon dates is that it does not provide a final definitive answer to the problem of converting radiocarbon dates to calendar years. Instead there are two separate diagrams: the calibration curve due to Suess which has appeared elsewhere, and the shape of which seems to have been arrived at intuitively rather than by any strict mathematical treatment of the data, and a diagram prepared by the editor on which the data of the other workers are plotted. Because this latter does not aim to compete with the other diagram by indicating a calibration curve, it serves principally to show how laboratories agree on the general trend of the large long-term variations and how they differ as to the position and magnitude of possible short-term fluctuations.

H. BARKER

CORRESPONDENCE

Definition of Cancer Research

SIR,—May I applaud the sentiment expressed in the article "What Price the Crusade for Cancer Research" (*Nature New Biology*, 230, 225; 1971) that "the pendulum of financial support should not be allowed to swing so far that cancer research becomes a synonym for the study of RNA viruses". On the other hand I am disturbed that the general theme appears to be merely that DNA viruses should also be studied. Perhaps no one is really advocating that the field should be so narrowly defined but it is evident that, because of so-called abuses, the range of projects supported by cancer research funds is to be severely restricted using relevancy as a basis for selection. As the *Nature* article implies, cancer research cannot succeed by virtue of its own advances alone: the more limited its outlook becomes, the more significant is this statement. A restrictive policy is, at best, acceptable only if adequate funds and facilities are available elsewhere so that all lines of research (and particularly those concerned with new concepts and fundamental issues) can still be accommodated. The present financial situation, however, is causing a general limitation on research: it is therefore essential to ensure that the studies supported by cancer research funds are those most likely to advance our comprehension of neoplasia and methods of treatment of the disease and hence that they are founded on correct concepts. It is therefore necessary, especially under present circumstances, to question the reasons behind, and the advisability of, the isolationist policy and the scientific grounds on which project selections may be made.

Abuse of funds arises primarily from the attitudes and motives of the clinicians and scientists concerned rather than the nature of their work. If it is an abuse to utilize or supply funds for fundamental studies or projects not directly associated with human tumours, then it is also an abuse to continue to pursue or support, say, a clinically orientated study which has failed to yield an effective means of therapy or is no longer likely to provide further and significant insight into the problem. Many studies based on conventional views are to continue despite the fact that they will probably do no more than amass relatively trivial facts and

may even cloud the real issues. The relevancy of a project is impossible to decide objectively (*cf* Poynter¹) as is its potential value, but surely it is better to attempt to apply the latter criterion if one must curtail projects. Again, novelty should be preferred to further variations on an outworn theme.

To my mind, anyone who proposes that the bulk of funds should be channelled into just a few lines of research is either displaying considerable (but unwarranted) faith or he is failing to acknowledge the complexity of the problem and the multifarious facets of the disease: for example, the many different forms in each of the many susceptible species; the extreme range in both the degree of malignancy and proliferation rates of tumour cells; the large number and variety of oncogenic agents (spontaneous initiation cannot be discounted); the fact that, in general, the individual properties of cancer cells (for example, glycolysis, respiration, proliferation rates, invasiveness) are not, in themselves, abnormal.

Failure to resolve a problem in the face of intensive and extensive research could indicate that the basic concepts are incorrect or that they are too rigid to allow a solution. It is possible that the malignant transformation reflects not one specific intracellular lesion but a change in the overall pattern of behaviour of the biochemical systems of the cell. Much of the research in this field appears to be based on the view that a genetic defect is involved (for example, a mutation, virome incorporation) although so far the evidence does not support this view. Theoretical considerations²⁻⁶ indicate that a given pattern of active genes or enzymes can be consistent with more than one stable metabolic state and hence it seems feasible that neoplasia is a result of transitions between conjugate states of this kind, probably affecting one or more biochemical sub-systems. In principle, such transitions could be induced by a variety of disturbances including, but not limited to, genetic changes. Reversal of a transition would not necessarily follow removal of the stimulus or be brought about by the processes of mitosis and cell division: hence the altered pattern of behaviour could be transmitted to daughter cells. Only if it were known that one particular type of agent alone induces all oncogenic transformations should attention be focused so dominantly on the properties of the agent:

since available evidence contradicts this kind of uniqueness, it would seem that we ought to concentrate attention on the response of the cell.

In addition to such considerations it is evident that we must understand the dynamics of cell biochemistry (normal or abnormal) if only to account for the cell proliferation features of this problem. Elsewhere⁷⁻⁹ I have briefly discussed, in general terms, the possible significance of cellular periodicities and suggested that the malignant transformation could reflect changes in the frequencies, phasings and so on of rhythmic biochemical variations (including supporting experimental data from a project now terminated as a result of a relevancy decision¹⁰⁻¹¹). Harington¹² has since considered a specific system in this light, *viz.* the Rapkine¹³ cycle involving thiol-disulphide rhythm, in relation to the cytokinetic aspects of malignancy. More recently, Sel'kov¹⁴ analysed the behaviour of the latter mathematically and decided that the thiol-disulphide system could exhibit two stable steady states each of which could be oscillatory. Transitions between these states might thus result in a change in the frequency of oscillation and hence altered proliferation rates. Moreover, Sel'kov points out that such a change, by virtue of the activation/inhibition action of thiols on various enzymes, could be expected to modify glycolysis, respiration etc., processes indicated by Warburg^{15,16}, Weinhouse¹⁷ and others as being concerned with the malignant transformation. I would like to take this view one stage further by pointing out that other cellular components (for example, metal ions) could, in principle, exhibit similar characteristics but be to some extent independent of the sulphydryl system. If this is so, then the overall behaviour of all these systems and their pattern of interaction could play a major part in determining the properties (and hence the normality or abnormality) of cells.

Clearly, if such views are basically correct, then it is possible to hinder advances by concentrating effort on a few special oncogenic agents or on a few narrowly defined and conventional areas of research. The unenviable task of those dispensing funds is very great: the onus is on them to make the right decision and they therefore deserve to hear the varied opinions of more of those actively engaged with the resolution of this problem. I would thus like to suggest

that an open forum be made available in your journal for the expression of opinions from all parties concerned. Incorrect decisions made at this stage could lead to even more marked and warranted charges of abuse being made at some later date.

Yours faithfully,

D. A. GILBERT

Goodmayes,
Essex

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British Diary

Wednesday, June 2

Some Psychiatric Aspects of Murder (5.30 p.m.) Dr Denis Leigh, University of London, at the Institute of Psychiatry, Maudsley Hospital, De Crespigny Park, Denmark Hill, London SE5.

Friday, June 4

Duodenal Ulcer—a Continuing Challenge (11 a.m.) Mr C. Wastell, University of London, at the Royal Postgraduate Medical School, Hammersmith Hospital, Du Cane Road, London W12.

Monday, June 7

The Role of Church's Thesis in Intuitionistic Mathematics (5.30 p.m.) Dr C. Cellucci, British Society for the Philosophy of Science, in the Joint Staff Common Room, University College London, Gower Street, London WC1.

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

- The Exploitation of Biological Signals. By Professor B. McA. Sayers. (Inaugural Lecture, 27 January 1970.) Pp. 123-156. (London: Imperial College of Science and Technology, 1971.) 40p. [54]
- British Launderers' Research Association. Annual Report 1969/1970 (Jubilee 1920/1970). Pp. 32. (London: British Launderers' Research Association, Hill View Gardens, NW4, 1971.) [54]
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- Ministry of Agriculture, Fisheries and Food. Agricultural Chemicals Approved Scheme. Approved Products for Farmers and Growers 1971. Pp. 160. (Pinner, Middx.: Ministry of Agriculture, Fisheries and Food, 1971.) [64]
- British Standards Yearbook 1971, complete to 31 December 1970. Pp. xxxii+823. (London: British Standards Institution, 1971.) £1.50 net. [64]
- Discovery Reports, Vol. XXXV, pp. 179-198: On *Ihlea magalhania* (Apstein) (Tunicata: Salpidae) and *Ihlea racovitzai* (Van Beneden). By P. Foxton. (London: Cambridge University Press, 1971.) £1.80; 55. [64]
- The British Petroleum Company Limited. Annual Report and Accounts for 1970. Pp. 60. (London: B.P., 1971.) [74]
- University of Oxford. Seventh Annual Report of the Delegates of the Science Area for the year ending 31 July 1970. Pp. 133. 50p. Ashmolean Museum—Report of the Visitors 1970. Pp. 66+21 plates. 75p. (Oxford: The University, 1971.) [74]
- Chatham House, and PEP. European Series No. 76: Agricultural Policy and the Common Market. By John Marsh and Christopher Ritson. Pp. 199. (London: Chatham House, and PEP, 1971.) £1.25. [74]
- Royal Scottish Museum: A Catalogue of Fossil Vertebrates in the Royal Scottish Museum, Edinburgh. Part 1: Actinopterygii. By I. G. C. Henrichsen. Pp. ix+102. (Edinburgh: Royal Scottish Museum, 1970.) [74]
- Biobits and Doré-Smith Atomunits: Principles of Model Construction. By Ivor Smith, Margaret J. Smith and C. Frank Doré. Pp. 48. (London: Capital Biotechnic Developments, 1971.) 40p. [74]
- The Macaulay Institute for Soil Research. 1969-1970 Annual Report. (No. 40.) Pp. 80. (Craigiebuckler, Aberdeen: The Macaulay Institute for Soil Research, 1971.) [84]
- Potato Marketing Board. Sutton Bridge Experimental Station—Annual Review—1970. Pp. 32. (London: Potato Marketing Board, 1971.) [84]
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- University of London. University College Annual Report, 1969/1970. Pp. 145. (London: University College London, 1971.) [84]
- Building Research Station Digest, No. 53: Project Network Analysis. Pp. 7. (London: HMSO, 1971.) 5p. [84]
- Unilever. Report and Accounts 1970. Pp. 48. (London: Unilever, Ltd., 1971.) [84]

Other Countries

- US Department of the Interior: Geological Survey. Professional Paper 306-D: Geology and Palaeontology of Canal Zone and Adjoining Parts of Panama—Description of Tertiary Mollusks (Gastropods: Eulimidae, Marginellidae to Helminthoglyptidae). By W. P. Woodring. Pp. iii+299-452+plates 46-66. \$2.25. Professional Paper 614-D: Pegmatitic Trachyandesite Plugs and Associated Volcanic Rocks in the Saline Range—Inyo Mountains Region, California. By Donald C. Ross. Pp. iii+29+1 plate. Professional Paper 642: Miocene Gastropods and Biostratigraphy of the Kern River Area, California. By W. O. Addicott. Pp. iv+174+21 plates. \$2.25. (Washington, DC: Government Printing Office, 1970.) [24]
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- Cimbebasia. Ser. A, Vol. 1, No. 7: Report on a Small Collection of Fishes from the Kunene River Mouth. By Mary-Louise Penrith. Pp. 165-176. Ser. A, Vol. 1, No. 8: Some Decapod Crustacea from Northern South West Africa, including a New Species of *Hippolyte*. By Brian Kensley. Pp. 177-188. Ser. A, Vol. 1, No. 9: The Constitution of the Intertidal Fauna of Rocky Shores of South West Africa, Part 1. Lüderitzbucht. By M. L. Penrith and B. F. Kensley. Pp. 189-240. Ser. A, Vol. 1, No. 10: The Constitution of the Fauna of Rocky Intertidal Shores of South West Africa. Part 2. Rocky Point. By Mary-Louise Penrith and Brian Kensley. Pp. 241-268. (Windhoek, SWA: State Museum, 1970.) [54]
- US Department of the Interior: Geological Survey. Bulletin 1322: Stripping-Coal Resources of the United States—January 1, 1970. By Paul Averitt. Pp. iv+34. \$0.25. Bulletin 1324-A: Changes in Stratigraphic Nomenclature by the US Geological Survey, 1969. Pp. iii+41. \$0.30. (Washington, DC: Government Printing Office, 1970.) [54]
- Smithsonian Contributions to Zoology. No. 80: Freshwater Triclad (Turbellaria) of North America. IV: The Polyparyngeal Species of Phagocata. By Roman Kenk. Pp. 17. \$0.30. No. 86: A Revision of the Genus *Zenoria* Mulsant (Coleoptera: Coccinellidae). By Robert D. Gordon. Pp. 22. \$0.35. (Washington, DC: Smithsonian Institution Press, 1970 and 1971. For sale by US Government Printing Office.) [54]
- Carnegie Institution of Washington. Year Book 69, 1969-1970. Pp. x+767. (Washington, DC: Carnegie Institution of Washington, 1971.) [54]
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- The Dental Care Program of the Los Angeles Hotel-Restaurant Employer—Union Welfare Fund—Summary Report of an Evaluation. By Jay W. Friedman. Pp. 46. (Bethesda, Md.: Dental Economics Section, Division of Dental Health, Bureau of Health Manpower Education, 9000 Rockville Pike, 1971.) [64]

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